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THE

AMERICAN JOURNAL

OF

SCIENCE AND ARTS.

CONDUCTED BY

rofessors B. SILLIMAN and JAMES D. DANA,

IN CONNECTION WITH

Professors ASA GRAY, and WOLCOTT GIBBS,
of CAMBRIDGE,

AND

rofessors S. W. JOHNSON, GEO. J. BRUSH, and
H. A. NEWTON, of NEW HAVEN.

SECOND SERIES.

VOL. XLII.—[WHOLE NUMBER, XCII.]
Nos. 124, 125, 126:
JULY, SEPTEMBER, NOVEMBER.

NEW HAVEN: EDITORS.
1866.

PRINTED BY E. HAYES, 426 CHAPEL ST.

3f

253551

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ERRATA.

P. 1, line 12 from bottom, for "Latham," read "Lapham."
P. 2, l. 16 from top, for "Alter," read "Altar."
P. 95, l. 22 from bottom, after the word have, insert "been."
P. 116, l. 10, for "Prof. Whiting," read "Prof. Whitney."
P. 117. l. 26, " " " " " "
P. 156—add as a foot note, to article by C. M. Warren, Cited from the Proceedings of the American Academy of Arts and Sciences for Jan. 31, 1866.
P. 170, line 10 from bottom, for "*root* of the tongue," read "*back* of the tongue."
P. 182, l. 8 from bottom, for "*Vowel o*^{ll}," read "*Vowel ö*^{ll}."
P. 271, l. 8 from bottom, for "Sparkler" read "Sharples."
P. 303, l. 19 from bottom, for "mére," read "mère."
P. 341. l. 16 from bottom, for "over 100," read "100."
P. 272, l. 4 from top, for "East Goshen, Chester, Co.," read "Low's Mine."
Vol. xli, page 379, line 3; for "$\sin(x-e_1-0^\circ 46')$" read "$\sin(x-\overline{e_1-0^\circ 46'})$."

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[SECOND SERIES.]

ART. I.—*Description of an Ancient Sepulchral Mound near Newark, Ohio;* by O. C. MARSH, F.G.S.[1]

IN the first volume of the Smithsonian Contributions Messrs. Squier and Davis have ably described the most important of those ancient monuments of the Mississippi Valley, which render that region so interesting to the student of American archæology. By discarding vague speculation, which had been the prominent fault of most previous investigators, and adopting that rigid method of research, inaugurated so successfully by Scandinavian antiquaries, these authors were enabled to embody in their work all that was valuable in previous accounts, and to add much new and important information concerning that ancient population of this country, who have left behind them so many imposing structures. The subsequent researches of Squier, Latham, and others, have thrown additional light upon this interesting subject, so that at the present time the "Mound-builders" can no longer be regarded as an unknown people, although both tradition and history are silent in regard to them.

Few of these ancient monuments of the West have attracted more attention than the group of 'Enclosures,' or 'Forts,' near Newark, Ohio, which have long been celebrated on account of their great extent, and remarkable regularity. They consist mainly of elaborate earthworks, in the form of a circle, octagon, and square, and enclose an area of about four square miles, on the upper terrace between two branches of the Licking River. They were well described by Atwater, in 1820, who regarded them as works of defense;[2] and subsequently by Squier and Davis, who, however, considered them sacred enclosures.[3] Scattered over the same plain, and crowning the neighboring hills, are numerous tumuli, or mounds, evidently erected by the same people that built the larger works.

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[1] Read before the Connecticut Academy of Arts and Sciences, Feb. 21, 1866.

While on a geological excursion through the West, during the last autumn, the writer spent several days at Newark, examining these various monuments in company with George P. Russell, Esq., of Salem, Mass., who is well versed in everything relating to American antiquities. In the course of our investigations a sepulchral mound was opened, which proved to be in many respects the most interesting one of the kind yet examined. Mounds of this class received from Squier and Davis much less attention than the smaller "Alter Mounds," as the latter usually contain more relics of ancient art. These authors, moreover, examined none of those belonging to the Newark group of works, although the mounds in that vicinity appear to present some points of difference from those of other localities. For these reasons a more detailed account of our explorations will be given than would otherwise be necessary. The mound selected for examination was about two and a half miles south of Newark, on the farm of Mr. Thomas Taylor, and was known in the neighborhood as the "Taylor Mound." It was conical in form, about ten feet in height, and eighty in diameter at the base, these being about the average dimensions of the burial mounds in that vicinity. It was situated on the summit of a ridge, in the midst of a stately forest. On the mound itself several oak trees, two and a half to three feet in diameter, were growing, and near them were stumps of others, evidently of greater age. The mound stood quite alone, nearly half a mile from its nearest neighbor, and about three miles from the large earthworks already mentioned. In our explorations we were greatly assisted by Dr. J. N. Wilson, and Messrs. Dennis and Shrock, of Newark, and Charles W. Chandler, Esq., of Zanesville, who are all much interested in the local antiquities of that region.

An excavation about eight feet in diameter was first made from the apex of the mound, and after the surface soil was removed the earth was found to be remarkably compact, probably owing to its having been firmly trodden down when deposited. This earth was a light loam, quite different from the soil of the ridge itself, and its peculiar mottled appearance indicated that it had been brought to the spot in small quantities. In excavating the first five feet, which was a slow and very laborious undertaking, nothing worthy of notice was observed except some traces of ashes, and pieces of charcoal and flint, scattered about at various depths. At five and a half feet below the surface, where the earth became less difficult to remove, a broken stone pipe was found, which had evidently been long in use. It was made of a very soft limestone containing fragments of small fossil shells, apparently Cretaceous species. No rock of precisely this kind is known to exist in Ohio. Pieces of a tube of the same material, and about an inch in diameter, were found near the pipe. The cavity was about two-thirds of an inch in diameter, and had been bored out with great regularity. Similar tubes have occasionally been found in mounds, but their use is not definitely known.

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[2] Transactions American Antiquarian Soc., vol. i, p. 126.

[3] Smithsonian Contributions, vol. i, p. 67.

About seven feet from the top of the mound a thin white layer was observed, which extended over a horizontal surface of several square yards. Near the center of this space, and directly under the apex of the mound, a string of more than one hundred beads of native copper was found, and with it a few small bones of a child, about three years of age. The beads were strung on a twisted cord of coarse vegetable fibre, apparently the inner bark of a tree, and this had been preserved by salts of the copper, the antiseptic properties of which are well known. The position of the beads showed clearly that they had been wound two or three times around the neck of the child; and the bones themselves, (the neural arches of the cervical vertebræ, a clavicle, and a first rib), were precisely those which the beads would naturally come in contact with, when decomposition of the body ensued. The remains evidently owe their preservation to this fact, as they are all colored with carbonate of copper, and the other parts of the skeleton had entirely decayed. The position the body had occupied, however, was still clearly indicated by the darker color of the earth. The beads were about one-fourth of an inch long, and one-third in diameter, and no little skill had been displayed in their construction. They were evidently made, without the aid of fire, by hammering the metal in its original state; but the joints were so neatly fitted that in most cases it was very difficult to detect them. On the same cord, and arranged at regular intervals, were five shell beads, of the same diameter, but about twice as long as those of copper. All had apparently been well polished, and the necklace, when worn, must have formed a tasteful and striking ornament.[4]

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[4] Native copper seems to have been the favorite material for ornaments among the mound-builders. The metal was, without doubt, derived originally from the Lake Superior deposits, although it may have been found in the drift. It was more probably taken directly from the deposits themselves, as they exhibit abundant evidence of ancient mining operations, which no one familiar with such matters would attribute to the more recent Indians.

About a foot below the remains just described, and a little east of the center of the mound, were two adult human skeletons, lying one above the other, and remarkably well preserved. The interment had evidently been performed with great care. The heads were toward the east, slightly higher than the feet, and the arms were carefully composed at the sides. A white stratum, similar in every respect to the one already mentioned, was here very distinct, and extended horizontally over a space of five or six yards, in the center of which the remains had been laid. The earth separated readily through this stratum, and an examination of the exposed surfaces showed that they were formed from two decayed layers of bark, on one of which the bodies had been placed, and the other covered over them. The smooth sides of the bark had thus come together, and the decomposition of the inner layers had produced the peculiar white substance, as a subsequent microscopic examination clearly indicated.[5] Directly above these skeletons was a layer of reddish earth, apparently a mixture of ashes and burned clay, which covered a surface of about a square yard. Near the middle of this space was a small pile of charred human bones, the remains of a skeleton which had been burned immediately over those just described. The fire had evidently been continued for some time, and then allowed to go out; when the fragments of bone and cinders that remained were scraped together, and covered with earth. All the bones were in small pieces, and most of them distorted by heat; but among them were found the lower extremity of a humerus, and some fragments of a fibula, which showed them to be human, and indicated an adult rather below the medium size. The two skeletons found beneath these remains were well formed, and of opposite sex. The ossification of the bones indicated that the female was about thirty years of age, and the male somewhat older. It is not impossible that these were husband and wife—the latter put to death and buried above the remains of her consort; and the charred bones may have been those of a human sacrifice, slain at the funeral ceremonies.[6] Near these skeletons was a small quantity of reddish brown powder, which proved on examination to be hematite. It was probably used as a paint.[7]

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[5] This white layer, which was thought by Squier and Davis to be the remains of matting, is a characteristic feature in burial mounds. It has only been found where the interments were unquestionably those of mound builders.

[6] Among the ancient Mexicans and Peruvians, when a ruler or other person of high rank died, his wives and domestics were often put to death at the tomb, and in some instances the remains were burned.

[7] A larger quantity of the same substance was found in another mound near Newark. May not the "iron rust" discovered in the mound at Marietta, and regarded by some as proof that the mound-builders were acquainted with that metal, have been merely this substance? Implements of hematite were, indeed, found in the same mound.—*Transactions American Antiquarian Soc.*, vol. i, p. 168.

On continuing our excavations about a foot lower, and somewhat more to the eastward, a second pile of charred human bones was found resting on a layer of ashes, charcoal and burned clay. But one or two fragments of these remains could be identified as human, and these also indicated a small-sized adult. The incremation had apparently been performed in the same manner as in the previous instance. Immediately beneath the clay deposit a third white layer was observed, quite similar to that just described. In this layer was a male skeleton, not in so good a state of preservation as those already mentioned, although evidently belonging to an individual considerably older. In this case also the head was toward the east, and the burial had been carefully performed. Near this skeleton about a pint of white chaff was found, which appeared to belong to some of the native grasses. The form was still quite distinct, although nearly all the organic substance had disappeared. A few inches deeper, near the surface of the natural earth, several skeletons of various ages were met with, which had evidently been buried in a hurried manner. All were nearly or quite horizontal, but no layer of bark had been spread for their reception, and no care taken in regard to arrangement of limbs. These skeletons were in a tolerable state of preservation, some parts being quite perfect. A tibia and fibula, with most of the corresponding bones of a foot, were found quite by themselves, and well preserved.

Our excavations had now reached the original surface of the ridge, on which the mound was erected, and we were about to discontinue further researches, when the dark color of the earth at one point attracted attention, and an examination soon showed that a cist, or grave, had first been excavated in the soil, before the mound itself was commenced. This grave was under the eastern part of the elevation, about four feet from the center. It consisted of a simple excavation, in an east and west direction, about six feet long, three wide, and nearly two deep. In this grave were found parts of at least eight skeletons, which had evidently been thrown in carelessly,—most of them soon after death, but one or two not until the bones had become detached and weathered. Some of the bones were very well preserved, and indicated individuals of various ages. Two infants, about a year and eighteen months old respectively, were each represented by a single os illium, and bones of several other small children were found. One skull, apparently that of a boy about twelve years of age, was recovered in fragments, and this was the best preserved of any obtained in the mound. The skeleton of an aged woman of small stature was found resting on its side. It was bent together, and lay across the grave with its head towards the north. Some of the loose, human bones, exhumed from the bottom of the grave, were evidently imperfect when thrown in. Among these was part of a large femur, which had been gnawed by some carnivorous animal. The marks of the teeth were sharply defined, and corresponded to those made by a dog or a wolf.

Quite a number of implements of various kinds were found with the human remains in this grave. Near its eastern end, where the detached bones had been buried, were nine lance and arrow-heads, nearly all of the same form, and somewhat rudely made of flint and chert. The material was probably obtained from "Flint ridge," a siliceous deposit of Carboniferous age, which crops out a few miles distant. These weapons are of peculiar interest, as it appears they are the first that have been discovered in a sepulchral mound, although many such have been carefully examined.[8] They show that the custom—so common among the Indians of this country—of burying with the dead their implements of war or the chase, obtained occasionally, at least, among the mound-builders. Not far from these weapons six small hand-axes were found, one of which was made of hematite, and the rest of compact greenstone, or diorite, the material often used by the Indians for similar articles. Two of these corresponded closely in form with the stone hand-axe figured by Squier and Davis as the only one then known from the mounds.[9] With these axes were found a small hatchet of hematite, a flint chisel, and a peculiar flint instrument, apparently used for scraping wood.

In the central part of the grave, near the aged female skeleton already alluded to, were a large number of bone implements, all exceedingly well preserved. Among these were five needles, or bodkins, from three to six inches in length, neatly made from the metatarsal bones of the common deer; and also a spatula, cut from an ulna, and probably used for moulding pottery. With these were found about a dozen peculiar implements formed from the antlers of the deer and elk. They are cylindrical in form, from three to eight inches in length, and an inch to an inch and a half in diameter. Most of these had both ends somewhat rounded, and perfectly smooth, as if they had either been long in use, or carefully polished. It is possible these implements were used for smoothing down the seams of skins or leather: they would, at least, be well adapted to such a purpose. A "whistle," made from a tooth of a young black bear, and several "spoons," cut out of the shells of river mussels, were also obtained near the same spot.

A vessel of coarse pottery was found near the western end of the grave, but, unfortunately, was broken in removing it. It was about five inches in its greatest diameter, six in height, and one-third of an inch in thickness. It was without ornament and rudely made of clay containing some sand and powdered quartz. It was filled with soft, black earth, the color being probably due to some animal or vegetable substance, which it contained when deposited in the grave. Fragments of a vase of similar material, but having the top ornamented, were found in another part of the mound. Neither of these vessels were superior, in any respect, to the pottery manufactured by the Indians.

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[8] Squier, Antiquities of New York, p. 331.

[9] Smithsonian Contributions, vol. i, fig. 110, p. 217.

Near the bottom of the mound, and especially in the grave, were various animal bones, most of them in an excellent state of preservation. Many of these belonged to the common deer, and nearly all the hollow bones had been skillfully split open lengthwise,—probably for the purpose of extracting the marrow,—a common custom among rude nations. Some of these remains of the deer indicated individuals of a size seldom attained by the species at the present time. Beside one of the skeletons in the grave, and evidently deposited with it, were several bones of the gray rabbit. This renders it not unlikely that the mound-builders used this animal for food,—a point of some interest, as the inhabitants of Europe in the stone age are supposed to have been prevented from eating the hare, by the same superstition that prevailed among the ancient Britons, and is still observed among the Laplanders.[10]

Some of the animal remains in the mound, although well preserved, were in too small fragments to admit of accurate determination. Characteristic specimens, however, were obtained of those in the following list:

Cervus Canadensis, Erxl., (elk).
Cervus Virginianus, Bodd., (common deer).
Ursus Americanus, Pallas, (black bear).
Canis latrans? Say, (prairie wolf).
Lepus sylvaticus, Bach., (gray rabbit).
Arctomys monax, Gm., (woodchuck).
Unio alatus, Say, (river mussel).

It will be observed that these are all existing species, and, with one or two exceptions, are still living in Ohio—a fact of some importance in its relation to the antiquity of the mounds. The discovery of these remains under such circumstances shows, moreover, that the mound-builders depended, to some extent, at least, on the chase for subsistence. If, however, they were a stationary and agricultural people, as is generally supposed, we should expect to find in the mounds, the remains of domestic, rather than of wild, animals, but none of these have yet been discovered. This may be owing to the fact that comparatively little attention has hitherto been paid to the animal remains, and other objects of natural history found in the mounds, although a careful study of these would undoubtedly throw much light upon the mode of life of the mound-builders.[11]

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[10] Lyell, Antiquity of Man, p. 24. London. 1863.

The excellent state of preservation of the various skeletons in this mound is remarkable, and has probably never been equalled in the hundreds that have hitherto been examined. The remains of undoubted mound-builders have almost invariably been found so much decayed that it was impossible to recover a single bone entire.[12] The preservation in this case was doubtless due in part to the excessive compactness of the earth above the remains, but mainly to the fact that the mound stood on an elevation, where moisture could not accumulate. The skeletons in the lower part of the mound were not so well preserved as those higher up, probably because the original soil of the ridge naturally retained more moisture than the earth above it. There may have been, moreover, a considerable interval between the irregular burials, and those that followed, and thus some of the skeletons commenced to decay before the mound was completed. The interval, however, could not have been of very long duration, as no perceptible deposit of vegetable matter was formed over the small mound then existing. The same may be said of the intervals between the regular interments, and also of the subsequent period preceding the final completion of the mound. It should, perhaps, be remarked before proceeding further, that this mound had evidently never been disturbed by the Indians, and that all the human remains and other objects found in it were undoubtedly deposited there by its builders. This will readily be admitted by every one familiar with the subject, as the last interment was at least seven feet below the surface, directly under the apex of the mound, and the white layers—infallible indications of regular burials of the mound-builders—all extended over the grave, and remained undisturbed.[13]

The skeletons found in this mound were of medium size, somewhat smaller than the average of those of the Indians still living in this country. The bones were certainly not stouter than those of Indians of the same size, although this has been regarded as a characteristic of the remains of the mound-builders. All the skulls in the mound were broken—in one instance pparently before burial—and most of them so much decayed hat no attempt was made to preserve them. Two, however, were recovered with the more important parts but little injured. Both were of small size, and showed the vertical occiput, promient vertex, and large interparietal diameter, so characteristic f crania belonging to the American race. In other respects here was nothing of special interest in their conformation. With a single exception, all the human teeth observed were perfectly sound. The teeth of all the adult skeletons were much worn, those of aged individuals usually to a remarkable degree. The manner in which these were worn away is pecuiarly interesting, as it indicates that the mound-builders, like the ancient Egyptians, and the Danes of the stone age, did not, in eating, use the incisive teeth for cutting, as modern nations do. This is evident from the fact that the worn incisors are all truncated in the same plane with the coronal surfaces of the molars, showing that the upper front teeth impinge directly on the summits of those below, instead of lapping over them. This peculiarity may be seen in the teeth of Egyptian mummies, as was first pointed out by Cuvier.[14]

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[11] The animal remains found near the Swiss lake habitations, show conclusively that the earliest inhabitants of those settlements were hunters, who subsisted chiefly on wild animals: at a later period, however, during the change to a pastoral state, domestic animals were gradually substituted as an article of food.—*Rütimeyer, Fauna der Pfahlbauten der Schweitz.* Basel, 1861.

[12] Squier and Davis regard this fact as evidence of the great antiquity of the mounds, as in England, where the moist climate is much less favorable for preserving such remains, perfect skeletons of the ancient Britons have been found, although known to have been buried at least 1800 years.—*Smithsonian Contributions*, vol. i, p. 168.

[13] It is well known that the modern Indians occasionally buried their dead in the mounds, but invariably near the surface; the position of such remains, and especially the manner of their interment clearly distinguish them from the original deposits of the mound-builders.

All the bones in this mound, animal as well as human, were very light, and many of them exceedingly brittle. They adhere strongly to the tongue, but application of hydrochloric acid shows that they still retain a considerable portion of the cartilage. Some of the more fragile bones, which showed a tendency to crumble on exposure to the air, were readily preserved by immersing them in spermaceti melted in boiling water, a new method, used by Prof. Lartet and other French paleontologists, and admirably adapted to such a purpose.

There are several points connected with this mound which deserve especial notice, as they appear to throw some additional light upon the customs of the mound-builders, particularly their modes of burial, and funeral ceremonies. One of the most remarkable features in the mound was the large number of skeletons it contained. With one or two exceptions, none of the burial mounds hitherto examined have contained more than a single skeleton which unquestionably belonged to the mound-builders, while in this instance parts of at least seventeen were exhumed. The number of small children represented among these remains is also worthy of notice, as it indicates for this particular case a rate of infant mortality (about thirty-three per cent) which is much higher than some have supposed ever existed among rude nations. Another point of special interest in this mound is the evidence it affords that the regular method of burial among the mound-builders was sometimes omitted, and the remains interred in a hurried and careless manner. This was the case with eleven of the skeletons exhumed in the course of our explorations, a remarkable fact, which appears to be without a precedent in the experience of previous investigators. It should be mentioned in this connection that nearly all of these remains were those of women and children. Their hurried and careless burial might seem to indicate a want of respect on the part of their surviving friends, were there not ample evidence to prove that reverence for the dead was a prominent characteristic of the mound-builders. It is not unlikely that in this instance some unusual cause, such as pestilence or war, may have made a hasty interment necessary. The various implements and remains of animals found with these skeletons also deserve notice, as they far exceed in number and variety any hitherto discovered in a single mound. They prove, moreover, that, if in this instance the rites of regular burial were denied the departed, their supposed future wants were amply provided for. The contents of one part of the cist (which is itself a very unusual accompaniment of a mound) appears to indicate that the remains of those who died at a distance from home were collected for burial, sometimes long after death. The interesting discovery of weapons, which were found with these detached bones, would seem to imply that in this case the remains and weapons of a hunter or warrior of disctinction, recovered after long exposure, had been buried together.[15]

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[14] Leçons d'Anatomie comparée, tome ii, p. 105. Bruxelles, 1838.

The last three interments in this mound were performed with great care, as already stated, and in strict accordance with the usual custom of the mound-builders. The only point of particular interest in regard to them is the connection which appears to exist between some of the skeletons and the charred human bones found above them. Similar deposits of partially burned bones, supposed to be human, have in one or two instances been observed on the altars of sacrificial mounds, and occasionally in mounds devoted to sepulture, but their connection with the human remains buried in the latter, if indeed any existed, appears to have been overlooked. Our explorations, which were very carefully and systematically conducted, clearly demonstrated that in these instances the incremation had taken place directly over the tomb, and evidently before the regular interment was completed: taking these facts in connection with what the researches of other investigators have made known concerning the superstitious rites of this mysterious people, it seems natural to conclude that in each of these cases a human victim was sacrificed as part of the funeral ceremonies, doubtless as a special tribute of respect to a person of distinction.

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[15] A similar custom still prevails among some tribes of western Indians.

All the skeletons in this mound, except one, appeared to have been buried in a horizontal position with the face upwards. The exception was the skeleton of the aged female found in the grave, which lay on its side; but this may have been owing to the fact that the body had been bent together, perhaps in consequence of age. The skeletons which had received a regular interment all had their heads toward the east, but no such definite position has been noticed in the remains found in other mounds. As the grave had the same direction, this can hardly have been unintentional, although it may have been determined by the position of the ridge upon which the mound stood. The layer of charcoal, not unfrequently found in sepulchral mounds, was wanting in this instance, as was also the evidence, usually afforded by the same substance, that the fire, which consumed the human remains, had been suddenly extinguished by a covering of earth. Possibly the former, as well as other objects of interest, were contained in the outer portion of the mound, which was not examined, although usually everything deposited by the mound-builders was placed near the center; and hence our explorations were chiefly confined to that part.

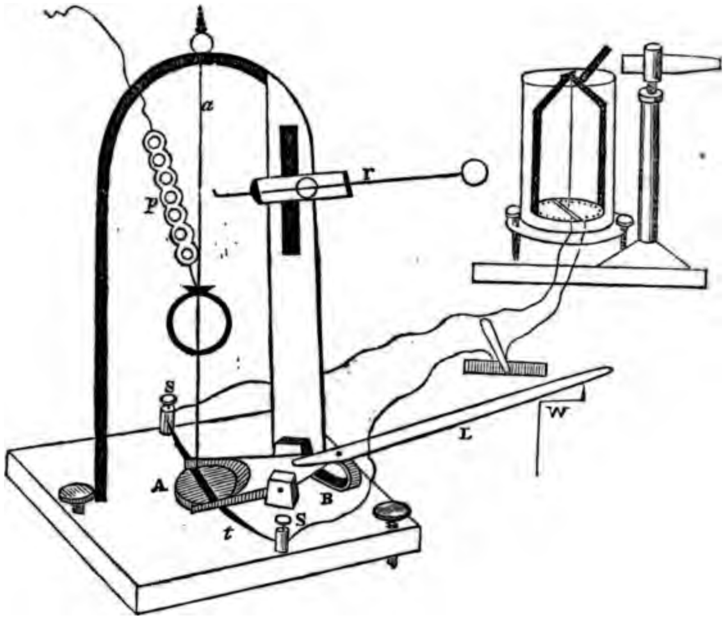
Such is a brief and incomplete description of one of the ancient mounds of the West, of which at least ten thousand are known to exist in the single state of Ohio, and countless numbers elsewhere in the valleys of the Mississippi and its tributaries. These structures are the only remaining memorials of a race whose history has been buried with them, and from these alone can we hope to learn who this people were, and whence they came. The Indians of this country, although retaining no tradition of this more ancient population, regarded their works with great veneration; but the present possessors of the soil have, in general, little of this feeling, and hence hundreds of these monuments of the past are annually swept away by the plow, and their contents irretrievably lost. A few pioneers in American archæology have, indeed, rescued much that is valuable, but the work is hardly commenced; and a careful and systematic investigation of these various monuments would not only add greatly to our knowledge of this interesting people, but doubtless also help to solve the question of the antiquity of man on this continent, and, perhaps, that more important one of the unity of the human race.

New Haven, Ct., Feb. 1866.

ART. II.—*On the production of Thermo-electric currents by percussion;* by O. N. ROOD, Prof. of Physics in Columbia College.

THE production of thermo-electric currents by *friction* was observed by P. Erman in 1845,[1] but I do not know that the subject of the present article has ever been examined with any care.

For the purpose of studying the thermo-electric currents produced by percussion, the apparatus represented in the figure was devised: it consists of a vertical brass wire *a*, stretched in the manner indicated; on it a brass ball weighing 17 oz. slides freely, the wire passing through one of its diameters. The ball can be raised to the height of from 1 to 5 inches by a string connected with the brass plate *p*. At the proper moment the ball can be allowed to fall: this is effected by passing the bent end of the rod *r* through one of the five holes in the brass plate *p*; by turning this rod through an angle of 90° the ball is set free and falls. The rod is fastened at such a height that when its bent end is in the highest of the five holes, the distance between the lower surface of the ball and the anvil below, is one inch. The holes in brass plate again are exactly one inch apart, so that the experimenter can easily, without altering the appa-



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[1] Arch. de l'El., v. 477; Inst. No. 614, p. 355.

ratus, obtain at will, a fall of 1, 2, 3, 4 or 5 inches successively, by raising the ball by the string, and using in turn each of the five holes in the brass plate. By this means the production of accidental thermo-electric currents from the heat of the hands is avoided, as the string and bent rod enable the observer to make the necessary adjustments from some distance.

The ball falls on a thermo-electric couple *t*, consisting of a compound wire of German silver and iron soldered together, or better, of a compound *plate* of the same metals, the juncture being soldered, as when *plates* are used, the couple suffers but little injury from the repeated falls of the ball. In the selection of these two metals for the couple, a suggestion of Poggendorff is followed, who showed that they give a strong current when their juncture is heated. The couple is so arranged that the ball strikes just on the juncture of the two metals, and there by means of the heat developed, produces a thermo-electric current. The juncture of the two metals was generally insulated by silk, &c., to prevent the heat from being immediately conducted off. The two farther ends of the couple were fastened by the binding screws *s s*, which were in metallic connection with a delicate galvanometer.

Below the couple is the brass anvil A.

A certain amount of heat is developed at the junction of the couple by a given fall of the ball; if now the couple were left in contact with the ball and anvil after the fall, this heat would be rapidly conducted away; it therefore became necessary to contrive, first, an apparatus for raising the ball instantly after its fall out of contact with the couple, and second, some arrangement for raising the couple at the same instant out of contact with the anvil. The former of these ends is accomplished by the lever L, the shorter arm of which is cut out so that when it is pressed down by the spring B, it rests on the anvil over the couple, and is out of the reach of the falling ball. As the sound of the concussion is heard, the long end of the lever is quickly pressed down, and fastened by turning the bent wire at *w*. The lever thus raises the ball ½ inch above the couple, and the latter itself acting at the same instant as a spring, raises itself by its own elasticity above the anvil. The wires from the binding screws were connected with an apparatus for breaking the circuit, in which small cups of mercury were used. This portion of the apparatus was placed on a table; the galvanometer, however, on a shelf attached to the wall of the room with brass nails, it being found that iron nails exercised a considerable effect on the astatic needle. When thus arranged, and observed with the telescope, the steadiness of the needle was not sensibly affected by a person walking about the room.

The upper needle of the galvanometer was provided with a very fine glass rod, which served as an index, the breadth of the rod being only half of that of the divisions on the galvanometer circle. The end of the glass rod was blackened to render it plainly visible. Directly over the needle, a mirror silvered by Liebig's process was placed at an angle of 45°; the index was observed with aid of this mirror and a small telescope magnifying five diameters; in this manner $\frac{1}{10}$° could be estimated.

The falling apparatus was enclosed by wooden screens, also the apparatus for breaking the circuit and the galvanometer. If these precautions are neglected accidental currents are constantly circulating in the wires employed, and no reliable results can be obtained. It is farther necessary after exchanging the couple or handling the binding screws, to allow the apparatus to remain at rest for two or three hours, so that the currents may subside; it is also necessary to select for observation, those intervals of time when the temperature of the room is constant. I may remark, finally, that in spite of all these precautions it is rarely the case that very feeble and nearly constant accidental currents are wholly absent.

The galvanometer was made by Duboscq; after balancing the magnetism of the needles it was found that the copper wire of the coil was so magnetic that the needles took up a position 30°–35° on either side of the zero point. I re-wound the frame with American wire, when the needle readily returned to the true zero; upon, however, bringing the two needles very nearly into the same plane, and carrying forward their astasie, the same difficulty was again experienced, when another sample of American wire was tried with a result which was but little better.

All of these samples when tested in the apparatus used for experiments on diamagnetism, were evidently magnetic, the French sample being strongly so. The difficulty was evaded by bending the needles slightly out of the true plane, when they took up a position nearly east and west, and returned with certainty to the true zero. In this state of inferior sensitiveness one simple oscillation consumed 18 seconds. There were sufficient indications to show that owing to the magnetism of the coil the needle was more sensitive to currents when standing at 10°–15° than when at 0°; it accordingly became necessary to calibrate the instrument with care. This was done by one of the methods described by Melloni and quoted by Tyndall, (Heat considered as a mode of motion, p. 370).

For degrees under 10° the constant currents employed in the calibration were produced by a small thermo-electric pile with one of its faces turned toward the exterior colder wall of the room, while the other face was directed toward an interior wall.

iese, as it were natural sources of heat gave very constant .rrents, and by partially closing one of the caps of the pile, ıy desired deviation between 0° and 10° could readily be obined.

It was found that for about 6° the deviation of the needle as directly proportional to the strength of the current; for deees beyond this, it was necessary to construct a curve embodyg the corrections obtained experimentally. The ratio between e first and final deviation up to 30° was also obtained; it was ınstant for 6°. These latter determinations were important, after the first deviation the needle, owing to conduction in e couple, slowly sinks to 0°, and only then comes to rest. I as not able to measure with exactitude the time required for ırrents produced by falls of the ball from different distances to ıbside, the imperfect results obtained showed that it varied beveen 1½ minutes up to 3½ minutes, according to the distance llen by the ball. It having been found then in the calibration speriments, that the force of the current was proportional to ıe deviation up to 6°, and farther, that the first deviation was roportional to the final deviation for the same number of derees, in the results given below, where the first deviation was elow 6°, the observations actually obtained and unreduced will e given, but where the first deviation exceeded 6° the reduced esults will be found.

As the total amount of heat produced by the fall of a body ; divided between the falling body and that arresting its motion, ; is evident that if the mass of the latter be small compared ith that of the falling body, its *temperature* will, owing to this ıct, be correspondingly high; and if the arresting body be a hermo-electric element of small mass, a proportionately large leviation of the galvanometer needle will be produced. If, ıowever, the couple at the moment of the percussion and aftervards, be allowed to be in metallic contact with the metallic ıall, the temperature of the couple will by conduction be rapidly educed to that of the metallic ball, so that the deviation of the ıeedle will be very small, and the phenomena complicated. To illustrate this I give, in table 1, the small and irregular deviations which were produced under these circumstances; the ball, couple, and anvil all remaining in metallic contact after the fall.

TABLE 1.

Distance fallen,	1 in.	2 in.	3 in.	4 in.	5 in.
Deviations,	$\frac{2}{10}^{\circ}$	$\frac{2}{10}^{\circ}$	$\frac{3}{10}^{\circ}$	$\frac{7}{10}^{\circ}$	$\frac{4}{10}^{\circ}$
	$\frac{1}{10}$	$\frac{2}{10}$	$\frac{3}{10}$	$\frac{3}{10}$	$\frac{5}{10}$
	0	0	$\frac{1}{10}$	$\frac{2}{10}$	0

In table 1, a newly prepared compound plate similar to that used in table 4 was employed.

To avoid the effects of conduction external to the couple, a number of insulating substances were tried, which gave more or less constant results.

1. Thin card board or plates of mica placed above and below the couple gave irregular results.

2. Two thicknesses of dried bladder, placed above and below the couple, gave somewhat better results.

3. Four thicknesses of heavy woven silk were also used for the same purpose.

4. The best results were, however, obtained by using heavy woven silk, which was spread over with a coating of yellow wax, and then wrapt around the couple at the juncture. This insulating substance after being used for some time so as to become compacted, gave results which were about as constant as could be expected under the circumstances.

Below are results obtained in these several ways:

Table 2.—*With two skins above and below.*

Distance fallen,	1 in.	2 in.	3 in.	4 in.
First unreduced deviations,	1·5°	2·2°	3·0°	5·8°
	1·4	2·3	4·3	4·1
	1·8	2·5	3·0	4·4
	1·2	3·5	3·4	5·9
	1·4	3·0	3·5	6·0
	1·2	3·5	3·7	5·0
	1·1	2·4	4·1	5·8
	1·1	2·2	3·4	4·9
Average,	1·3	2·7	3·55	5·2

In this and in all the other tables, the order of the experiments was across the page, from left to right, and not down the single columns.

Table No. 3, contains results obtained with four layers of plain silk above and below the couple.

Table 3.

Distances fallen,	1 in.	2 in.	3 in.	4 in.
First unreduced deviations,	1·6°	3·4°	3·7°	6·1°
	1·4	2·7	3·9	5·4
	1·6	3·5	4·1	6·0
	1·7	2·7	4·2	6·0
	1·5	2·6	4·3	6·0
	1·5	2·8	5·0	6·5
	1·4	2·6	4·7	6·0
	1·4	2·6	4·3	6·9
Average,	1·5	3·0	4·5	6·0

The results given in tables 2 and 3, were obtained by using a compound *wire* of German silver and iron with a diameter of 9 of a millimeter; the juncture was bound with a little fine iron wire and soldered. This form of couple was found to lose its shape by the repeated blows; it also finally cut the insulating substance, so that in all the following experiments *plates* of the metals soldered together were used; the form of these remained nearly unaltered.

ordingly, to obtain the results given in 4 and 5, a com- plate of this kind was used; the breadth of the plate millimeters, length 150 millimeters, thickness of iron and rman silver being about ·2 of a millimeter.

plate in table 4 was wound with four layers of heavy silk.

TABLE 4.

Distance fallen,	1 in.	2 in.	3 in.	4 in.	5 in.
st unreduced deviations,	1·6°	2·6°	3·8°	4·0°	4·9°
	1·4	2·3	3·4	3·9	4·6
	1·3	2·2	3·1	3·9	4·7
	1·3	2·4	3·4	4·0	4·5
	1·3	2·2	3·5	3·8	4·5
Average,	1·38	2·3	3·4	3·7	4·6

ble 5 gives results when 4 layers of *waxed* silk were used the same couple.

TABLE 5.

Distance fallen,	1 in.	2 in.	3 in.	4 in.	5 in.
irst unreduced deviations,	1·5°	3·1°	5·0°	6·4°	8·9°
	1·5	3·0	4·8	6·3	8·7
	1·6	2·9	4·8	6·2	8·2
	1·5	2·9	4·2	5·6	8·3
	1·4	2·8	4·2	5·7	8·0
Average,	1·5	2·94	4·6	6·04	8·4
Reduced average,	1·07	2·1	3·28	4·3	6·0

will be observed that in tables 2, 3 and 5 the result is more s perfectly indicated that the force of the current is pro- onal to the distance the ball falls through, or in other words e square of its velocity at the moment of impact.

of allowing the ball to remain in contact with insulated couple after the impact.

e results given in table 6 were obtained directly after those in table 5, everything remaining unaltered except that all was not raised out of contact with the couple.

TABLE 6.

Distance fallen,	1 in.	2 in.	3 in.	4 in.	5 in.
rst unreduced deviation,	2·0°	2·5°	3·6°	4·2°	5·8°
	1·9	2·6	3·1	4·0	5·7
	1·7	2·3	3·2	4·0	5·4
	1·6	2·3	3·0	3·6	5·1
Average,	1·8	2·42	3·22	3·95	5·5

extent to which the heat generated is thus conducted from the couple is very noticeable in the last three columns, is a little remarkable that the deviations in the first col- are higher than in table 5; a corresponding result obtained with a *narrow* couple is given below in table 8. To ascertain that the plate had not been altered, experiments were made with it afterward, the ball being lifted.

To find out whether any peculiar influence was exercised by the mass of the couple within small limits, the above mentioned plate was now cut down, till its breadth was 3 millimeters. It was covered with waxed silk and the following results obtained:

TABLE 7.

Distance fallen,	1 in.	2 in.	3 in.	4 in.	5 in.
Reduced deviations,	1·50°	3·28°	4·80°	6·46°	7·20°
	1·35	2·42	4·77	6·10	7·10
	1·20	2·42	3·50	4·90	6·38
	1·14	2·07	3·00	5·00	7·00
	1·07	2·01	3·00	4·90	6·46
	1·14	1·90	3·20	5·00	6·20
Average,	1·23	2·35	3·71	5·39	6·72

To compare the temperature here developed with what was produced in the broad plate before it was cut down, I give below the reduced deviation of the broad plate taken from table 5:

	1 in.	2 in.	3 in.	4 in.
Reduced deviation of narrow plate,	1·23	2·35	3·71	5·39
" " " broad "	1·07	2·1	3·28	4·30
Difference,	·16	·25	·43	1·09

The narrow plate used in table 7 being employed and arranged exactly as before, the ball was allowed after its fall to remain in contact with the insulated couple.

TABLE 8.

Distance fallen,	1 in.	2 in.	3 in.	4 in.	5 in.
Unreduced deviations,	2·4°	3·0°	3·7°	3·9°	5·6°
	2·5	3·1	3·8	3·9	5·9
	2·5	3·0	3·4	3·5	5·2
	2·6	3·1	3·7	3·9	5·9
Average,	2·5	3·05	3·6	3·8	5·65
Reduced average,	1·79	2·17	2·57	2·7	4·0

Effect of the first twenty, &c. falls on the newly prepared plate.

When the couple is wound with plain or with waxed silk, and subjected to the action of the falling body, the first 15–20 deviations of the needle are much larger than any above given with the successive falls as the silk becomes compacted, the deviations decrease in size, reach a minimum, and remain about as constant as shown in the tables. The results so far given then, except, of course, in case of table 1, were obtained after this point had been approximately reached. I give below, as a sample, the first set of deviations obtained directly after winding with waxed silk the couple used in table 7. The ball was lifted in the usual way.

Distance fallen being 5 in., the reduced deviations are given: 23·5°, 17·2°, 16·3°, 13·8°, 12·4°, 11·6°, 10·1°, 8·5°, 9·1°, 9·1°, 8·6°, 8·4°, 8·1°, 7·9°, 8·4°, 8·4°, 7·85°, 7·4°, 7·5°, 6·6°, 7·2°, 7°, 6·8°.

A similar action was observed with unwaxed silk. This might be accounted for by saying that the mass of silk and wax becoming compacted is then a better conductor of heat than before, and that the temperature of the couple is thus lowered by the short but necessary contact with the ball; but the comparatively small effect which is produced even by continued contact with the ball shown in tables 6 and 8 prove that this supposition is untenable.

The larger deviation must then be attributed to the sliding of the particles of silk and wax over themselves, this taking place to a much greater extent in the first twenty falls than afterward. After the minimum point has been reached, if the couple is laid bare and rewound, the same large deviations are obtained, showing that they are not due to an alteration in the couple itself.

Finally, it is remarkable that a much smaller mechanical force applied directly to the couple in the shape of friction, produces a disproportionately large deviation; thus drawing the wooden end of a lead-pencil once over the naked junction with a force less than would be generated by the ball falling 1 inch gave a deviation of 18–25°.

It is hardly necessary to add, that the deviation of the needle was in all cases in the same direction as though heat had been applied to the juncture of the thermo-electric couple.

New York, Feb. 22, 1866.

ART. III.—*A Classification of Mollusca, based on the "Principle of Cephalization;"* by EDWARD S. MORSE.[1]—With a plate.

AFTER becoming acquainted with the perfect unity of plan in the Radiata and the connected series of homologies, running through the whole branch, (as demonstrated by Prof. Agassiz in his private lectures) my interest was excited to discover, if possible, a like symmetry of development in the Mollusca. Finding the universality of vertebration among the Vertebrata, of articulation among the Articulata, and similarly of radiation among the Radiata, I could not but believe that in the Mollusca some plan lay hidden, which, when unfolded, would as definitely convey their type, and unite them all, as in the other branches,

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[1] Proc. of Essex Institute, iv, p. 162.

It is not enough to call them soft bodied animals; for in considering their shell as a part of their organization, we have among them many of the hardest animals known, and we also have an equal number of soft bodied animals in the other branches. Their bilaterality, as expressing anything definite, is an equally unsatisfactory character. Prof. Huxley has given an archetype, or common plan of the Mollusca, as he conceives it, with many truthful homologies, in the article "Mollusca," English Cyclopedia, vol. iii, p. 855. In his figure of the archetype, however, which is bilaterally symmetrical, we have details of structure only.

Prof. Agassiz in his "Methods of Study in Natural History" also suggests his idea of the plan, or structure, when he says, p. 34, "Right and left, have the preponderance over the other diameters of the body," and says furthermore, that collectors unconsciously recognize this in the arrangement of their collections. "They instinctively give them the position best calculated to display their distinctive characteristics, and to accomplish this they necessarily place them in such a manner as to show their sides." This can refer only to the Lamellibranchs, and their shells are displayed on the sides, because they naturally fall in that position. This lateral preponderance of structure only obtains among the Lamellibranchs. All Brachiopods are displayed from the dorsal or ventral valve. Also the Gasteropods, particularly the flat forms like Patella, Chiton, etc., and the Nudibranchs as well, while in the figures of the naked Cephalopods we most usually have a dorsal view.

Though Prof. Agassiz speaks of radiation as characterizing the Radiates, and similarly of articulation and vertebration as characterizing the Articulates and Vertebrates, yet Mollusks are spoken of as first introducing the character of bilaterality, or division of parts along a longitudinal axis, that prevails throughout the Animal Kingdom, with the exception of the Radiates. This then can be no restricted definition for the Mollusca, since it pervades the two higher branches; and who will deny the evidence of bilaterality among the Radiates, the higher Echinoderms for instance, as Clypeastroids and Spatangoids, where we have as good a definition of a longitudinal axis as we obtain in many Mollusks. Even among the Polyps, as in the Actinaria, the antero-posterior axis is clearly expressed in the undue prominence of the primary radii.

Prof. Dana has been the first to publicly announce the plan of Mollusca, when he says, "The structure essentially a soft, fleshy bag, containing the stomach and viscera, without a radiate structure, and without articulations."[2]

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[2] Dana's Manual of Geology, p. 148.

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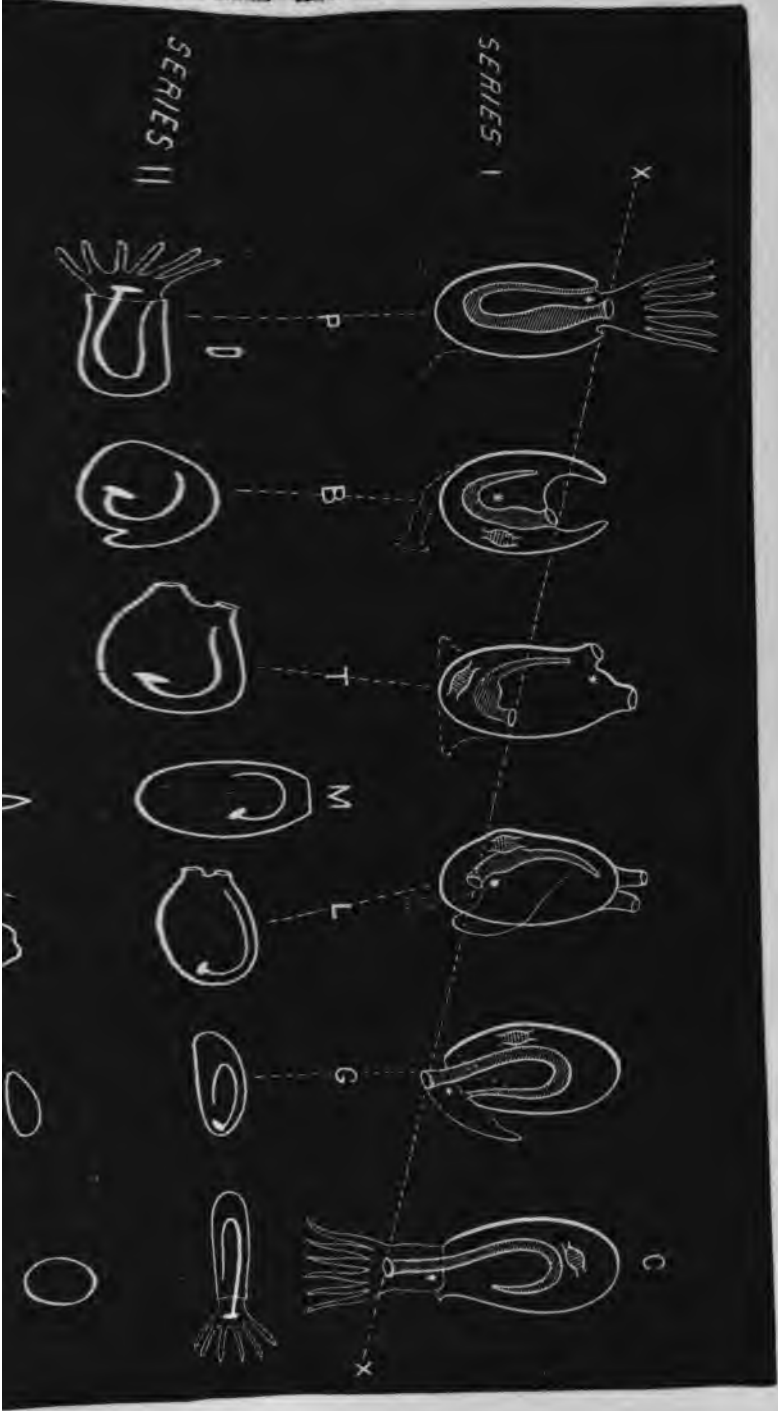
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As far back as 1855 he presented this thought in his lectures at Yale College.

In the year 1862 Mr. Alpheus Hyatt had independently worked out a similar result, and has already in MSS. notes, the necessary data demonstrating the same.[1]

Mr. Hyatt also proposes the name Saccata as more fully and truthfully expressing the type, than the unmeaning word Mollusca. This name not only expresses the Plan, but is equivalent to the titles Vertebrata, Articulata, and Radiata, and is in no way a qualitative appellation.

Objecting as all must to the introduction of a new name, still one so appropriate as that proposed by Mr. Hyatt, in lieu of one that has no relation to the Branch, except its traditional use, is certainly worthy of consideration, as it so clearly indicates what is believed to be the fundamental idea in the Branch, that of the Sac.

* * *

In the following considerations, all preconceived ideas regarding the relative positions of the dorso-ventral, and antero-posterior diameters of the animal must be laid aside, and the essential structure of the animal, if rightly understood, must be our guide. The gradual morphological changes of the contents of the sac, and all other relations, are based on the principle of Cephalization. In the plate presented (Series I) I have given a typical figure of the six prominent groups of the Saccata; namely, Polyzoa, Brachiopoda, Tunicata, Lamellibranchiata, Gasteropoda, and Cephalopoda.

For obvious reasons, only the intestine, head, and pedal ganglia within the sac are represented. These six figures are placed in their normal position, anterior pole downward, the dorsal region is turned to the left. Commencing with the Polyzoa, (Series I, P) we have the sac closed, while the mouth and anus terminate close together at the posterior pole of the sac; the mouth occupying the extreme posterior position, and by a dorsal bend of the intestine upon itself, terminating dorsally. The nerve mass is found between the oral and anal openings. In this class the mouth and anus have the power of protrusion from the sac. In the three lower orders, Cyclostomata, Ctenostomata, and Cheilostomata, the polyzoon, when completely evaginated, presents no fold or inversion of the sac, while in the higher group Phylactolæmata, there is a partial and permanent inversion of the sac under like conditions.

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[1] Mr. Hyatt has relinquished all ideas of publishing on this subject, since becoming aware that I was to do the same. During the preparation of these pages, I enjoyed his companionship, and many of the points herein stated were fully and freely discussed between us, and to him I am indebted not only for the privilege of announcing his proposed name, Saccata, but for the suggestion of certain points to be hereinafter mentioned.

This latter group, combining the permanent inversion of the sac-walls with the lophophoric arms, is the first approach to the Brachiopoda. No organ corresponding to a heart has yet been discovered. In the Brachiopoda (Series I, B) we have a permanent invagination of the sac, and the mouth, as in Terebratula, already occupies a position some distance from the posterior edges of the overlapping shells, and the brachial coils permanently occupy the space thus made.[4]

We have in this group a dorsal flexure of the intestine, and a tendency to terminate as in the polyzoa. In Lingula it terminates posteriorly and at one side. By the permanent inversion of the sac, the mouth makes a great advance toward the anterior pole. In Terebratula, Waldheimia, and allied genera, where the sac is very short and swollen, and the brachial coils very large, the viscera are crushed to the front, and the intestine, which is short and simple, is nearly bent upon itself, though still occupying a median line. In Lingula, where we have a very long and flat sac, the intestine is long, and has ample room for convolutions, but the anus, instead of terminating in a line with the mouth, is thrown to one side, in consequence of this excessive flatness of the sac. The heart will be found on the outer bend of the intestine and actually on the ventral side; the nerve occupying its homological position.

(The manner in which I view the Brachiopoda, if true, will entirely reverse the accepted poles of their structure. What has been considered as dorsal, is here regarded as ventral, and what has been considered as anterior is here regarded as posterior. Further remarks on this will be made hereafter).

Thus far the balance of structure has been thrown to the posterior pole of the sac, and though we see a cephalization, or concentration of the muscular system and viscera, toward the anterior pole in Brachiopoda, yet that pole being essentially closed, we have no function manifested at that end, except the degradational one of adhesion. In the Tunicata (Series I, T) we have, through continued cephalization, the mouth thrown to the bottom of the sac, or nearer the anterior end, and now the anus terminates behind the mouth, and posteriorly.

The heart has also followed the intestine in its rotation and becomes anterior, and partially dorsal. The nerve mass is still posterior and occupies a position between the two openings as in Polyzoa. We have commencing in this group, the Tunicata, that erratic bending of intestine, and varied position in its anal termination, that is witnessed higher up in the scale, and though apparently governed by no law, we can yet trace the progressive movements toward a normal condition, by comparing Appendicularia, one of the lowest forms of the Tunicates, and representing the larval condition of their class. In this form the intestine has a ventral flexure, and terminates on the ventral side. In Pyrosoma it makes an abrupt bend toward the anterior dorsal region, and terminates anteriorly. In Salpa it terminates dorsally, on a line with the mouth, though still anteriorly. In Botryllus it creeps up, and terminates nearer the posterior pole of the sac, though still dorsally. We have in this genus, and other compound Ascidians, the excurrent orifices of several individuals coalescing, forming a common cloaca for a community. The dorsal flexure is distinctly seen in Clavellina borealis. In these three classes; namely, Polyzoa, Brachiopoda, and Tunicata, the sac is essentially closed at the anterior end, and consequently the mouth opens toward the posterior end, and with few exceptions all are attached by the anterior end.

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[4] "Terebratulina caput-serpentes, and Crania anomala, projected their cirri beyond the margin of the open valves, and moved them as the Polyzoa move their oral tenticles, but in no instance were the arms extended." Woodward's Treatise, p. 466.

This makes a natural division, corresponding to the Molluscoidea of Milne-Edwards, the Anthoid Mollusks of Dana, and a portion of the neural division of Huxley. In the Lamellibranchiata (Series I, L) we have the sac opening anteriorly, and the mouth permanently occupying the anterior region, though in the lower forms pointing posteriorly, and in all cases the tentacular lobes pointing in that direction, and the mouth bent downward (ventrally), and partially obstructed by the anterior adductor, or by the undivided mantle. The gradual enlargement of the anterior opening is clearly seen, where in the Gastrochœnidæ, we have first a minute orifice for the passage of an immature foot, or metapodium, this opening gradually enlarging in different genera, until in the Unionidæ we have the sac almost completely separated, except dorsally. It will be noticed that the anterior opening is also ventral, or nearly so, in the lower forms. In Gasteropoda (Series I, G) the posterior end of the sac becomes essentially closed, and the ambient fluid now finds access to the gills through the anterior (though partially ventral) portion of the sac, while with Cephalopoda (Series I, C) the opening is all anterior. Thus far we have traced the gradual cephalization of the contents of the sac, and of the sac itself. The dotted lines X X, running through the oral opening of each figure in Series I of Plate, show the gradual advance of this opening from the lower to the higher classes. In the lowest class all the display of structure, with the oral and anal openings, lies at the posterior pole of the sac. In this highest class, all this display of structure lies at the anterior pole. Advancing from the Polyzoa, by the gradual advance of the mouth, the posterior pole becomes less prominent. Even when the sac opens anteriorly, as in the Lamellibranchiata, the posterior end of the sac remains open, and the mouth, partially inclined that way, receives its food from that end; the food being conducted to the mouth by ciliary motion as in the three lower classes. The nature of their food is also identical, being of an infusorial character, and as such it is obvious that masticating organs, or biting plates, such as we find in the two higher classes, are not needed.

So long also as the posterior end of the sac remains open, the anus terminates at that end; when this opening becomes closed, as in the higher classes, the anus seeks an outlet through the anterior opening, and the mouth, that before received its food from the posterior end of the sac and by ciliary motion, now distinctly points the opposite way, and is furnished with the proper organs to procure food, the nature of which requires separation and trituration.

In nearly all the foregoing homologies, and also the position in which I place the Tunicate sac, I am sustained by the writings of eminent naturalists. With the Brachiopoda, however, my views completely reverse the accepted poles of the body, though, even here, according to "Woodward's Treatise on Mollusca," page 204, Forskahl and Lamarck "compared *Hyalea* with *Terebratula*; but they made the ventral plate of one answer to the dorsal valve of the other, and the anterior cephalic orifice of the pteropodous shell correspond to the *posterior*, byssal foramen of the bivalve!" And, if the views I advance prove correct, they were precisely right. In all my previous attempts to homologize the different classes, I had always met with an obstacle in the apparently aberrant characters of the Brachiopods: never for a moment doubting the truth of the accepted views, that indicated the regions to be called dorsal and ventral, as such, I labored in vain. When I undertook to interpret the relation of these classes on the principle of cephalization I found that these accepted views must be doubted, and it was with amazement that I beheld such unlooked for results: that the so-called anterior pole is really the posterior pole, and that the so-called dorsal region is really the ventral region.

It has not been without patient consideration that I now advance these views, knowing that by many they will be received with opposition: nevertheless, the more I try to make them comformable with already received relations, the more I am convinced that such relations are wrong; and it is only in believing that continued research will but confirm these propositions, that I now dare to offer them.

According to the views here advanced, the Brachiopods are (1) attached by a prolongation from the *dorsal* area, as in the lower Polyzoa, where they lie on the back. (2) In their natural position in life, this valve is really uppermost. (3) The process of attachment also proceeds from the anterior pole of the body, as in all the members of the branch even to Gasteropods, with the exception of those attached by one valve (e. g. Ostreans, Clavagella), whether it be by a byssus, confined in cells of their own making, or buried in the mud, it is the anterior end which is fixed. In several lower forms, like Tridacna and Anomia, the point of attachment springs from the dorsal area, as in the two lowest classes. In regard to the posterior position of the mouth in Polyzoa and Brachiopoda, we have similar analogies among the Articulata; Cirripedia, for example, where we have animals becoming attached head downward, and all the oral parts, as in the pedunculated forms, tending toward the posterior pole of the body; or in Limulus, where we have such a decephalization, as it were, that the mouth occupies nearly a central position in the ventral region.

Again, considering the intestine as a simple tube, opening at each end, with the weight of structure evenly divided between the two openings, is it any more incredulous, that the oral opening should be posterior, than that the anal opening should be anterior, as in the Gasteropods?

In Polyzoa, the oral and anal openings occupy a similar position in all the forms. In Brachiopods, while the mouth remains in nearly a constant position, the anus terminates either in a median line, or by a lateral deflection of intestine to one side. In Tunicata, while the mouth occupies a permanent position at the front of the sac, the anus terminates at various portions of the sac, generally in a median line, though there is usually a lateral deflection of the intestine.

In Lamellibranchiata, the mouth and anus terminate in a median line, with few exceptions, (e. g. Pecten) though the intestine convolutes in various ways. In Gasteropods we have again lateral deflection of intestine, and though in many genera the anus terminates in a median line, yet in the bulk of the Gasteropods it terminates at one side or the other. In the Dibranchiate Cephalopods we have again the termination of the intestine in a median line.

The diagram here given (fig. 1) represents an ideal longitudinal section of the sac, similar to those of Series I. The arrow within the sac shows the direction of rotation of the bent intestine, carrying with it the heart, (see Plate, Series I,) which in Brachiopoda we find on the ventral region; in Tunicata on the anterior dorsal region; in Lamellibranchiata on the dorsal region; in Gasteropoda on the dorsal region and also further back; and in the Cephalopods at the posterior portion of the sac. The different positions of the sac

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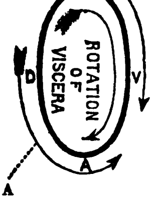

openings (represented in fig. 1 by arrow O) follow the same direction, that is from posterior to anterior, ventrally. Thus in Tunicata the two openings are posterior and posterior-dorsal; the posterior-dorsal, being the anal or excurrent orifice; this is always the shortest in Tunicata. In Lamellibranchiata the anal tube moves nearer the branchial tube; in the lower forms their outer covering coalescing and of equal length, while, higher up, the tubes become entirely separate, and, in some, of extreme length, the anal tube being the longest. In Pisidium and other forms the branchial tube disappears, and water is received through a ventral opening; while the anal tube yet remains, occupying a posterior position on a line with the antero-posterior axis, in the same position the branchial tube occupied in the Tunicata: and, finally, both tubes become nearly obsolete, and the mantle is cleft all round, except dorsally. Thus the progress of sac opening follows in the same line of rotation with the intestine. The progressive regions of attachment move in an opposite direction (fig. 1, arrow A). Commencing with the Polyzoa as the lowest class, we have, as in the Cheilostomata, the dorsal portion large and spreading, this being the fixed portion; the anal opening being turned toward this region, as in the Brachiopoda and Tunicata, (the movable part of the ventral surface, which is uppermost, being represented by the little lid). This mode of attachment is the lowest feature; namely, attached along the entire dorsal region.

As we ascend to the higher forms of the class, we have a freeing of the posterior portion of sac, and the viscera permanently occupies this freed portion. In the Brachiopoda we have the sac free, held only by the peduncle; the means of attachment springing anteriorly, and from the dorsal valve, as in the partially freed polyzoon. Crania and Discina are attached as in Lepralia.

In Lingula, where we have the lengthened and flattened sac, the animal stands vertical in the sand. In Terebratula and allied genera, the dorsal valve already assumes preponderance over the ventral valve, and now obtains its normal position uppermost.

All the Tunicates with few exceptions are attached, and by their anterior end.

In the compound Ascidians like Botryllus, where we have a community of individuals clustering round a common center, their dorsal as well as anterior regions are attached, or, in other words, the ventral and posterior regions are free only.

Among the Lamellibranchiata nearly all the lower forms, and many of the higher forms, are fixed or stationary; and whether moored by a byssus, buried immovably in the mud, or imprisoned in cells of their own making, it is the anterior end which is fixed. This obtains, with important exceptions.

The Monomyarians combine in their structure both high and low characters. In their open mantle, and certain other features, they rank high. In their fixed position, the attachment generally springing from the dorsal region, they rank low. For these reasons, I have placed them in the center (see Plate, Series II, M) not indicating by this their equal value with the other groups, for I doubt if their separation from the Dimyarians is valid, since the large adductor, composed of two elements, would indicate the presence of both anterior and posterior adductors, combined in consequence of the excessive shortness of their antero-posterior diameter. The Monoyarians present singular features of analogy with the Brachiopoda. Thus they are generally inequivalve. The viscera are compacted toward the dorsal region, and, when attached, they are generally by a process from the dorsal portion (e. g. Anomia), the lowest feature of attachment. In all these instances, particularly with Anomia, the analogy is very striking; it is analogy only, and nothing more, for in their whole structure, and in the relative proportion of their diameters, they present just the opposite extreme. While we have in Brachiopoda the growth laterally, that is, spreading on the sides and depressed dorsally, and the valves, dorsal and ventral, in the Monomyarians we have the other extreme; the valves are right and left, and the display is on the side, the growth extending ventrally as it were. So narrow are they that in certain forms, Placuna for example, it is almost impossible to conceive the presence of soft parts between the valves. We compare the relative diameters between the Brachiopods and Monomyarians, to show how unlike they are in this respect.

Diameter.	BRACHIOPODS.	MONOMYARIANS.
Antero-posterior.	Medium.	Small.
Dorso-ventral.	Small.	Very large.
Transverse.	Large.	Very small.

By reason of their excessive narrowness, the greater number of Monomyarians lie on the right or left valve, and as their peculiar form precludes the possibility of locomotion by the usual organ, the foot, they either remain fixed, or swim freely about in the water, by violently closing their valves, as in Lima and Pecten.

Among the Unionidæ, the highest family in the Lamellibranchiata, the animal assumes nearly a horizontal position in crawling, though the anterior end is always the lowest, and generally buried in the mud. Its embryos, like Monomyarians in shape, are attached to the ovisac by the dorsal margin, which is straight, as in Pecten. (Lea's paper on Embryonic forms of Unionidæ, Journ. Acad. Nat. Sci., 2d Series, vol. iv, plate 5.)

By their violent shutting of the valves, while in embryo, they may, after birth, swim, even as Pecten swims; at all events they are said to become attached by a byssal thread while young. Among the Gasteropods we have a few genera attached, or fixed, as in Magilus, Siliquaria, Vermetus, Spiroglyphus, Nerinæa, and Petaloconchus. These are now attached posterior end downward. In Calyptræa they are in a fixed position, secreting a ventral valve, upon which they rest. It would be interesting to know for a certainty which part first becomes attached in Vermetus and allied forms; their first point of attachment must take place at the mouth of the tube or aperture, which is really anterior and ventral. The Cephalopods are free.

Thus we have the various regions of attachment, changing and following in the direction indicated by the arrow A, in figure 1.

1st, Polyzoa: dorsal attachment.
2d, Brachiopoda: dorsal and anterior attachment.
3d, Tunicata: anterior.
4th, Lamellibranchiata: anterior and ventral attachment.
5th, Gasteropods: ventral and posterior attachment.

While we have thus seen that the area of attachment first springs from the *dorsal* region, and gradually changes as we ascend in structure toward the anterior end, so we find the principal organ of locomotion, i. e., the foot, is first developed from the *ventral* region, and in like manner tending toward the anterior end, as we ascend in the scale, until, in Cephalopoda, the specialized divisions of the foot surround the head, and point directly forward.

Having personally communicated the substance of this paper to Professor James D. Dana, he has, in a letter to me, indicated certain gradient relations among the Lamellibranchs, Gasteropods, and Cephalopods, as manifested in the special characteristics of the head, or anterior part of the body, so clearly illustrating the principle of cephalization that I now take the liberty of presenting them. In the Lamellibranchs the foot is a simple muscular organ developed from the ventral surface and protruding anteriorly. It is simply an organ of locomotion, in the lower forms not even performing this function. The oral opening is a simple slit, without the power of seizing or triturating its food.

In the Gasteropods the foot is more specialized, and as an organ of locomotion far superior to that of the Lamellibranchiates, having oftentimes three well characterized regions, called by Huxley, the pro-, meso-, and metapodium, these regions oftentimes supporting certain processes, e. g., cirri, opercula. The foot not only performs locomotion but in many cases has the power of siezing and retaining its prey (e. g., Natica). The mouth has an apparatus for biting and triturating its food, being furnished with an upper jaw, or buccal plate, and a tongue, armed with siliceous particles. In the Cephalopoda the foot is so far differentiated as to be separated into prehensile arms furnished with rows of suckers, or hooks. These arms surround the head, and are thrown directly forward. They are capable not only of locomotion, but of seizing their prey, and performing also movements of aggressive action. In the higher forms of Cephalopods, the function of locomotion is delegated to other organs, while the arms subserve the uses of the head alone, and the mouth, furnished with two powerful mandibles opposed vertically, forcibly reminds us of a parrot's beak, or that of certain other vertebrates. Thus we have cephalic power manifested in the mechanical action of the foot.

1st, Lamellibranchs—Locomotion.
2d, Gasteropods—Locomotion, Prehension.
3d, Cephalopods—Locomotion, Prehension, and Aggression.

According to the principle of cephalization, cephalic power is manifested either as a mechanical, sensorial, or psychical force. Thus the Cephalopods possess in the greatest measure, all three; while Gasteropods, not indicating, to any great extent, aggressive action, may be said to manifest but little psychical power; and the Lamellibranchiates manifest essentially only mechanical action.

We have based the preceding considerations on the common structure of each class, and for comparison have given an archetype, as it were, of each class (Series I). In continuing these archetypal figures, as illustrating the relative diameters and mean forms for each class (Series II and III), and also the mean, or average position in nature of the antero-posterior axis (Series IV), we obtain singular features of polarity,[5] which I will now proceed to indicate; premising, however, that what follows is offered in reluctance, as I have not at present the opportunity to verify the statements as I would wish. In Series II the average lateral form of each class is given. In Series III a transverse section is given of the same figures in Series II. In Series II the arrow A indicates the direction of posterior pole, and D indicates the dorsal region in Series II and III. In Series IV a line for each class is given, representing the average position of their antero-posterior axis in nature (A, anterior pole, P, posterior pole). The central figures in Series II, III, and IV represent corresponding views of the Monomyarians. In the Polyzoa (Series II, P) the sac is long and cylindrical, the mouth and anus terminate at the posterior pole, and the tentacles surround the mouth only; the anus terminating outside the lophophore. Witness in the highest order of Cephalopods, the Dibranchiates, the sac as in Loligo (Series II, C), long and cylindrical, and in all cases mouth and anus opening anteriorly; the arms surrounding the mouth only. Two rough diagrams, alike in form, but reversed in one case, would represent each class as we have it here. In Brachiopoda (Series II, B) we have the sac widening laterally, and correspondingly depressed dorsally; mouth and anus opening posteriorly. In Gasteropoda (Series II, G) we have the same features, except that the parts are reversed again. In Tunicata (Series II, T) the sac is lengthened and swollen. Lamellibranchiata (Series II, L) the same. The relative diameters of the Monomyarians are unlike those of any other class, as before pointed out.

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[5] We use this word in its most general sense.

It is confidently believed that when these relations or polarities, between the ascending and descending, or, as Professor Dana terms them, the Holozoic and Phytozoic classes, have been further studied, new and interesting features will be revealed. Thus, the resemblances between the Tunicates and Lamellibranchiates are too obvious to indicate.

Among the Brachiopods and Gasteropods, beside what has been pointed out, we have unlooked for similarities, as for instance Discina and Calyptræa, or Terebratula and Hyalæa. Among the Polyzoa and Cephalopoda, though no polarities are brought to mind, except those given above, yet we cannot help remarking how strong the resemblance is between the Polyzoa and Protozoa, through Vorticella: and if Vorticella belongs to Polyzoa, as Professor Agassiz appears inclined to believe, a few steps more bring us to the Ammonitic forms of the Rhizopods. This is speculative (though suggestive), as it is now considered by many that the Protozoa forms a fifth Sub-Kingdom.

In considering transverse sections of the sacs, as shown in Series III, we obtain a like order of polarity. Thus the highest orders in Polyzoa and Cephalopoda presents a circular section. Brachiopoda and Gasteropoda are transversely oval; Tunicates and Lamellibranchiates are longitudinally oval, or in lower forms circular; while the Monomyarians have the dorso-ventral diameter in excess, and the transverse diameter reduced to the minimum.

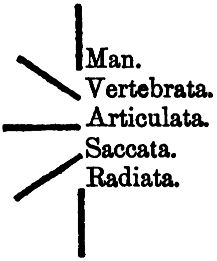
In considering the position, or angle of the antero-posterior axis of each class in nature, we obtain similar results (Series IV).

Polyzoa and Cephalopoda, we place in a horizontal position, taking a swimming Dibranchiate for comparison: this may be premature however.

Brachiopods and Gasteropods with posterior pole slightly elevated, as in Cyrtia and allied forms of Brachiopods, and any coiled Gasteropod for example. Tunicates and Lamellibranchiates with the axis vertical, the anterior pole being below, and the Monomyarian horizontal again. It must be remembered that the above considerations are taken in their most general sense, representing only the mean for each group, many of them perhaps erroneous. They are given rather for the purpose of indicating a further path of inquiry, which the writer considers fruitful and intends to follow, than as points in any way settled.

In ascertaining the mean position of the antero-posterior axis for the whole branch of Saccata, (that is, the average) we find that a line at an angle of 45° would represent its position in nature; the lower end being anterior. In the Radiates a line through the mouth to the opposite region of the body would stand vertically. In Articulates the antero-posterior axis would be horizontal. Among the Vertebrates, Fishes would be horizontal, as in Articulates; Reptiles have the head slightly elevated; Birds and Mammals still more elevated; so that a mean line, for these classes might be drawn at an angle of 45°, the cephalic region being uppermost. Man stands vertical. Thus in a diagram we would have the following:

2.



In the preceding considerations I have endeavored to show the importance of the sac, as the principle and prominent feature in their plan of structure. All animals, reduced to their primary elements, are sacs in one sense of the word, though in one case a radiate sac, in another an articulate sac, etc. Yet nowhere does this character predominate so universally, nor is it expressed so simply as in the Mollusca; the leading idea as it were. It was shown also that, essentially, the heart is on the outer bend of the intestine, or between that and the sac wall, while the principal nerve mass was on the inner bend of the intestine. We would thus state their characters.

SACCATA.

(1.) *Animals of varied forms, without a radiate structure and without articulations.*

(2.) *Stomach and viscera enclosed by a fleshy sac, which may be closed or open, at either one or both ends.*

(3.) *Principal nerve masses, consisting of ganglia, which are adjacent to, or surround the œsophagus.*
(4.) *Intestine bending inward, or having an outward flexure.*
(5.) *Heart on the outer bend of intestine.*

SACCATA.	HOLOZOIC, OR TYPIC. *Mouth opens anteriorly.*	Sac open at anterior end.	CEPHALOPODA. GASTEROPODA.
		Sac open at both ends.	LAMELLIBRANCHIATA.
	PHYTOZOIC, OR HEMITYPIC. *Mouth opens posteriorly.*	Sac open at posterior end.	TUNICATA.
		Sac closed.	BRACHIOPODA. POLYZOA.

We must now consider the relations of the Saccata to the other branches of the Animal Kingdom. In the paper of Professor Dana, above referred to, he has used the terms alphatypic, betatypic, and gammatypic, as a numbering of the grade of types, whether of branches, classes or orders; also, below gammatypic, we have degradational; the Radiates are regarded as degradational; and below this, hemiphytoid. He employs also, the terms used above, namely, Holozoic, for true animal forms, and Phytozoic, for plant-like forms.

Applying these terms to the classes or groups of Saccata, we have the following:

HOLOZOIC.	Alphatypic,	CEPHALOPODA.
	Betatypic,	GASTEROPODA.
	Gammatypic,	LAMELLIBRANCHIATA.
PHYTOZOIC.	Degradational,	TUNICATA. BRACHIOPODA.
	Hemiphytoid,	POLYZOA.

Prof. Dana has pointed out many interesting parallelisms between the groups of the different branches. Let us now look at the parallelisms between the groups above indicated, and the other branches. Cephalopods approach nearest the Vertebrates through their lowest class, the fishes, and already many interesting analogies have been pointed out between them.

Gasteropods may be likened to Articulates, through their lowest class, the Worms, through certain resemblances that many forms bear to the Leeches, Planarians, and Trematodes. Lamellibranchiates may be considered the essential embodiment of the branch to which they belong. Tunicates and Polyzoa may be compared to Radiates.

Or, in considering their freedom or fixedness in life, we have Cephalopods free, as in all Vertebrates; Gasteropods, a few fixed, as in Articulates; Lamellibranchiates, many fixed, as in Saccata, with relation to the other Branches. Tunicates, the greater portion fixed, though they do not compare so well with the Radiates in this respect, but Brachiopods and Polyzoa fixed as in the lowest class of Radiates, the Polyps.

We would thus have

Alphatypic,	*Cephalopods,*	*Vertebrates,*	Fishes.
Gammatypic,	*Gasteropods,*	*Articulates,*	Worms.
Betatypic,	*Lamellibranchiates,*	*Saccates.*	
Degradational,	*Tunicates,* *Brachiopods,*	*Radiates.*	
Hemiphytoid,	*Polyzoa,*	*Radiates,*	Polyps.

EXPLANATION OF THE PLATE.

Series I. Represents a typical figure of each principal group in Mollusca—viz., P, Polyzoa; B, Brachiopoda; T, Tunicata; L, Lamellibranchiata; G, Gasteropoda; and C, Cephalopoda—(M, indicating Monomyaria of the second series). These figures are represented anterior end downward, the dorsal region being turned to the left. The tube within each cut, represents the intestine, the larger end of which is the mouth, and the smaller end the anus. The harp-shaped figure represents the heart, and the star represents the pedal ganglion.

Series II. Represents similar views, with less detail. The dorsal region in this series is uppermost, and the posterior end is turned to the left, as indicated by arrow A. The curved line indicates the intestine, the large end being the mouth.

Series III. Represents transverse sections of corresponding figures in Series II.

Series IV. Represents the mean position in nature, of the antero-posterior axes of the figures represented above: A, Anterior pole; P, Posterior pole. The vertical rows of figures are identical.

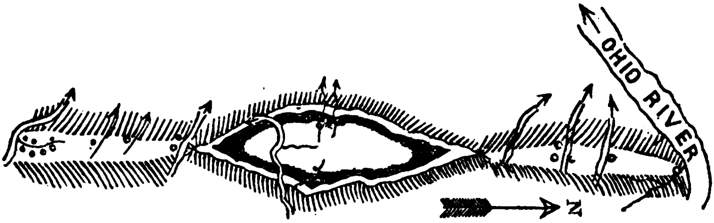
Art. IV.—*Petroleum in its Geological Relations;* by Prof. E. B. Andrews, Marietta, Ohio.

In the number of this Journal for July, 1861, I gave some facts bearing upon the geological relations of petroleum. My attention at that time was confined chiefly to those locations found in the Coal-measures of West Virginia and Southern Ohio. It is gratifying to know that the views presented in that paper have since been fully verified. As predicted, by far the larger part of the oil produced has been found along the axis of a well-marked anticlinal, extending from the borders of southern Ohio, forty miles or more, into West Virginia, through Wood, Ritchie and Wirt counties. A smaller quantity has been found in the inclined rocks of Ohio; while scarcely a barrel has been obtained in horizontal rocks, although hundreds of thousands of dollars have been expended in the search. In this portion of our great Coal-measures the question has been solely one of subterranean fissures. The chemical conditions essential to the generation of oil have existed over a wide area; but the physical condition of fissures is found to exist in comparatively limited areas. Fissures serve two purposes, one to give space for the formation and expansion of the hydro-carbon vapors, and the other to furnish receptacles for the oil when condensed. These fissures must connect with the deeply seated sources of the oil.

If they have any surface outlets, by which the more volatile portion of the oil may escape as gas, the oil within them is thickened and lowered in specific gravity. As the escape varies greatly from different fissures, we find oil of every grade of gravity. I have known oil of 52° from a well one hundred feet deep, and also oil of 28° from the bottom of one eight hundred and fifteen feet deep. The Scott well on White Oak, Wood Co., West Virginia, struck, at two hundred and seventy-four feet, a fissure containing oil of 33°, and, at three hundred and ninety-one feet, another fissure yielding an abundant supply of oil of 27½°. Hence, while, as a general rule, oil found near the surface is heavy, the fissures containing it being more likely to have surface outlets, yet sometimes the very deep fissures may have such outlets, and the contained oil be heavy.

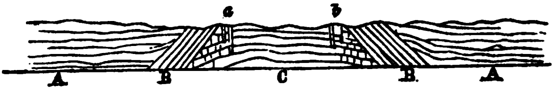
The West Virginia oil field presents many points of great scientific interest. All the productive oil wells in this part of the State group themselves along the anticlinal line marked out in the article referred to, this line being the one of the greatest fissuring of the rocks. Toward its northern and southern extremities this line presents the form of a simple anticlinal with the rocks dipping on either side of the axis at angles varying from 5° to 25°. But in the middle part there is a double fracture, the lines of dislocation inclosing a somewhat elliptical-shaped area about ten miles long by one wide. These figures are only proximate estimates. A bird's-eye view would present an appearance somewhat like that given in fig. 1. The more important oil locations are indicated by the marks o.

1.



A section of the dislocated strata is given in fig. 2.

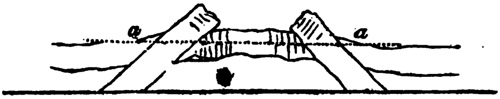
2.



A, A represent the horizontal rocks. These belong to the highest strata of the Coal-measures. B, B represent the dislocated strata, inclining in opposite directions at angles varying from 30° to 60°. Without having made any instrumental measurements I have estimated the thickness of these strata at about eight hundred feet. C gives the position of strata lying within what is popularly called "the break." These rocks belong to the lower Coal-measures and have been more or less flexed by lateral pressure. It is in these middle rocks that the most valuable wells of West Virginia are now being obtained. Wells bored in the rocks A, A, have been failures as also the wells bored in B, B. The rocks B, B, appear to have been lifted up bodily, and in such a way as not to have been much fissured. The advantages of the inner strata, marked C, as oil-producing rocks, are: first, they are bent and more or less fissured; second, they are many hundred feet lower in the series than the strata at A, A, and are consequently so much nearer the equivalents of the supposed sources of oil in the Devonian rocks of Western Pennsylvania and Canada; and third, this local disturbance of the rocks doubtless involves in its many fissures these underlying Devonian strata, and thus has given every opportunity for the generation of oil and its upward ascent. I think we may reasonably infer that the oil found along this line is of the same origin geologically as the oil obtained in the upper Devonian rocks of Venango Co., Pa. Thus far the oil obtained within this double fracture has been found very near the inner edges of B B, as represented by the italics, *a* and *b*. These small letters indicate, respectively, the geological positions of the "White Oak" and "Mount's Farm" wells on the western, and the Handlan wells on the eastern side. The Volcanic Oil Co. and the West Va. Oil and Oil Land Co. own large areas of land within the "breaks." The "Mount's Farm" and other companies own smaller tracts.

I cannot but regard the term "volcanic" as infelicitous when applied to this region. Nothing is more sensitive to heat than petroleum, and direct igneous action adequate to the work of uplifting and dislocating the strata to this extent would, I think, have driven off all the oil. The uplifted strata at C (fig. 2) contain seams of bituminous and cannel coal which possess the normal and average quantity of bitumen. There is, to my mind, a much better and more scientific explanation of this disturbance, one which assigns the cause to the lateral pressure produced by the subsidence of all the rocks of this region of country. This line of local uplift is found to be in the very heart or center of the synclinal part of our great coal field and at the summit of the coal formation. From forty to sixty miles both to the west and east the underlying strata, with their productive coal seams, begin to emerge. It would appear that at some time after the deposition of the last of the strata of our upper and barren Coal-measures there was a local subsidence which necessitated such lateral pressure as would cause an uplift and dislocation. In this way alone could the sinking strata make room for themselves. For the most part along the line there was a pretty sharp anticlinal formed, but in the center of the line there were two fractures which may, I think, be satisfactorily explained. A popular illustration of the dislocation would be the case of ice fractured and heaped up by the lateral pressure of currents. It would be easy to find two cakes uplifted and forced upon a central one as represented in fig. 3; the central cake at the same time being forced upward and cracked by the force which wedges it in. If the top of the projecting mass were planed off down to the dotted line *a*, *a*, we should have in the ice phenomenon a perfect representation of the rocks in West Virginia as shown in fig. 2. We should expect that the ice would be the most fractured near the edges of the central cake and where the pressure is most direct. In like manner, experiments thus far made show that the most oil fissures have been found in C (fig. 2) near the more vertical rocks B, B. The finding of more oil near the edges of C may also be explained in part by the sloping position of the impervious strata B, B, causing the oil, which in its upward ascent might strike them, to be forced into the adjacent fissures of C. While the chances of striking fissures are doubtless greater in the rocks just within and near the lines of dislocation, yet when more extended explorations are made, I have no doubt that oil will be found in the more central portions of C. It is a fact of much interest that almost all the oil thus far obtained in these central rocks is heavy oil and found comparatively near the surface. The wells range from 20 feet to 400 feet in depth. The Harkness well yields 200 barrels a day of heavy oil from a fissure 164 feet deep. The Longmoor wells find oil in large quantities at the depth of 265 feet. The Atwater well, 198 feet deep, yields 300 barrels a day.

3.

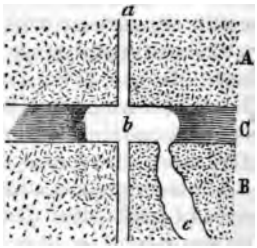


It should be remarked that this line of uplift has no parallelism whatever with the general range of the uplifted Alleghany mountains, but on the other hand, makes with that range an angle of about forty degrees. Hence it is difficult to suppose the two uplifting forces to have been one and the same. I have no doubt that the vertical fissure on McFarland's run, a few miles east, which is now filled with asphaltum (a fact which came to my knowledge many years ago) was produced by the same force that dislocated the rocks under consideration, and at the same time. The denuding action along this line of uplift must have been very great, as, in some places, not less than a thousand feet of the Coal-measures have been eroded. I can, however, find no traces of any other agency of erosion than those now at work, viz., atmospheric and aqueous. From West Virginia this line of disturbance passes into Ohio, crossing the Ohio river near Newport, Washington Co., O., but, farther to the north, the evidences of disturbance gradually fade away. The lateral force expends itself in producing some smaller parallel undulations. The Newton well on Cow run, a branch of the Little Muskingum river, is exactly in the anticlinal axis of one of these smaller undulations. This well began to flow in June, 1861, and is, I believe, still flowing. It is 200 feet deep.

I have thus discussed the relation evidently existing between lines of geological disturbance and the production of oil in West Virginia and southern Ohio. A similar connection has been observed by Sir Wm. E. Logan in the oil fields of Canada (Geology of Canada, p. 379). The oil obtained on the upper Cumberland river in southern Kentucky has been found, so far as I can learn, in locations of similar disturbance.

But there is another and very important class of facts to be noticed in connection with the subject of the geology of oil. We find in many parts of the country a very marked tendency in the oil to accumulate in certain geological horizons. The stratigraphical position of most of the oil in southern Ohio (in the Coal-measures) is in a vertical range of about two hundred feet of rocks lying below the horizon of the Pomeroy coal seam. This is true in Meigs, Athens, Morgan, Noble and Washington counties. There are some exceptions to this rule, but they are few. On Big Sandy river in Kentucky, the conglomerate below the coal is the "oil rock." In Scioto and Pike counties in Ohio there is a well marked horizon of oil springs in the Waverly sandstone, within twenty feet of its line of junction with the underlying black shale. At Mecca, in Trumbull Co., O., there is a similar and well-defined "oil rock." But the most notable fact of this kind is observed in Venango Co., Penn., where, on Oil creek, Cherry run, and Pit-hole, the oil is chiefly obtained in the fissures of the "third sand-rock." This rock is reached at a depth of from eight hundred to a thousand feet below the base of the Coal-measures. Coal is mined in the hills adjacent to Pit-hole creek. No oil, so far as I could learn when investigating that region, has been obtained in the arenaceous shales below the third sand-rock, although a few very deep wells have been sunk. No sand-rock was found below the third. The oil is found in the third sand-rock, not because it is the third, but because it is the *lowest*, and as such has intercepted the oil in its upward ascent. I should here remark that the third sand-rock is in some places divided into two parts by about two feet of soft shales, popularly called the "mud rock," and the lower part is sometimes called the fourth sand-rock. Oil is sometimes obtained in the mud rock. This is readily explained by the following figure (fig. 4). A and B are the upper and lower divisions of the sand-rock; C is the "mud rock" penetrated by the well *a*. The shale is softened by the water in the well, and enters the well in the form of mud. A small cavity, *b*, is thus formed, which sometimes extends to an oil fissure, *c*, and thus a good oil well is obtained..

4.



At Tideoute, on the Alleghany river, a fine oil field, the famous Economite wells struck the lowest sand-rock about one hundred and forty feet below the surface. Very deep wells have been bored in the neighborhood without finding any lower sand-rock. I had little doubt, when examining the region, that this sand-rock served the same purpose as the third sand-rock on Oil Creek; it intercepted and retained in its fissures the oil. When a fissure chanced to extend to the surface, the usual phenomena of oil and gas springs were seen. Such oil springs first called attention to that region. It would therefore appear to be a geological law in the upper Devonian rocks of western Pennsylvania that the lowest impervious sand-rock retains in its fissures the oil. In the same way the hard and compact lower strata of the Waverly sandstones of southern Ohio intercept the oil as it rises from the bituminous shales below. Some of this oil finds an outlet through fissures extending laterally to the surface of the outcropping rock. Whether by boring, at points removed from the outcrop, where there could have been no surface drainage, large quantities of oil may be found, remains to be seen. If sufficiently capacious fissures in this oil-horizon exist, I have no doubt that they will be found to contain large quantities of oil.

I would, in passing, venture to express my dissent from the opinion of some geologists, that oil which may have been formed in higher strata descends to lower. In all my investigations of this matter I have never found any evidences of such a fact, while, on the other hand, the natural tendency of oil is upward: the waters lift it up; its cognate gases often force it up; the original oil vapors rise to condense in higher and cooler cavities; and the oil which had first been condensed from vapor in lower fissures may often be re-volatilized, to ascend and find higher places of condensation near the surface. This last mentioned process may have been going on in many regions for an indefinite period. It is possible even, that in some localities all, or nearly all, the oil has been brought up from its deep birthplaces to very near the surface. In such localities very deep wells would avail nothing. It is certainly evident that on Oil Creek, Pit-hole, &c., the oil has come from below and accumulated in the fissures of the lower sand-rock. It could not have been forced down from strata above. Confining the winds in bags were an easy task compared with forcing *down* and shutting up the oil with its furious gases in the cavities of the third sand-rock. Nor could the oil have travelled in currents horizontally from the coal rocks on the southeast. This would imply that the oil penetrated diagonally the sandstones of the upper Devonian, which dip to the southeast, or that it descended vertically a thousand feet to the base of the third sand-rock and then moved under this cover to the northwest. These suppositions are entirely untenable. The oil is found in independent fissures, is of different specific gravity in different localities, and is accompanied by mineral waters varying in chemical constituents and combinations. Such facts forbid the supposition of great lateral subterranean movements. The same reasoning would apply to the theory sometimes published, that oil passes down the long slopes of gradually descending strata to the lowest part of synclinal basins, and there accumulates. Strong brine in an open permeable rock can thus descend, but not oil. If the brine carried the oil down with it, we should expect to find in the salt wells of Pomeroy, O., no little oil with the brine. The brine there is obtained in the conglomerate a thousand feet down. When oil is obtained it is from fissures comparatively near the surface.

Of the origin of the oil fissures in the sand-rocks on Oil Creek, Pit-hole, Tideoute, &c., in western Pennsylvania, I cannot speak with entire confidence. The whole region is covered with drift materials hiding the underlying rocks. There are doubtless undulations in the strata caused by the general uplifting force which gave the rocks their dip. The productive wells appear to group themselves along certain belts between which are barren intervals, and as these belts seem to be parallel with the undulations, it is probable that the oil-producing fissures are limited to the axial lines of these undulations.

The limits of a single article forbid my considering other oil fields. There are some other localities of great promise, but to discriminate beforehand these from those of no value is a most difficult task. A little oil is to be found almost everywhere in our country where the rocks are not metamorphic, and in almost every geological formation, from the Lower Silurian upward. I have seen samples of oil from nearly every western and southwestern state. I have myself found it in every stage of the change from light fluid oil to hard asphaltum. A fine sample of bitumen, received from California twelve years ago, interested me much as containing many very old bones. This specimen prepared my mind to receive statements subsequently made, that wild animals were sometimes mired and died in the tarry oil springs in that State.

Of the origin of petroleum there are different opinions. All agree, however, that it must ultimately be traced to vegetable or animal substances, the primary combinations of hydrogen and carbon being the product of vital force. It is the opinion of Dr. J. S. Newberry and others that petroleum in its present form is the product of a slow distillation of bituminous strata. From this theory Mr. T. S. Hunt of the Canada Survey, in the "Geology of Canada," p. 526, dissents, and quotes approvingly the views of Mr. Wall, who investigated the bitumens of Trinidad, and who writes that the bitumen "has undergone a special mineralization, producing a bituminous matter instead of coal or lignite. This operation is not attributable to heat, nor of the nature of a distillation, but is due to chemical reactions at the ordinary temperature and under the normal conditions of climate." It would appear to be Mr. Hunt's opinion that the bitumens, of which petroleum is the liquid form, are the product of chemical reactions changing the original organic materials directly into oil and kindred hydrocarbons. The facts cited in proof are, that oil is found in the cavities of fossils (*Orthocerata*, &c.), and in thin strata composed of certain corals, and in similar cases, where the oil must have originated in the places where found and directly from the organic materials. I have observed many similar facts, particularly in the Devonian limestones of Ohio. These facts are conclusive so far as they go. There is no doubt that at the original bituminization of organic matter vast quantities of bitumen were formed. The greater portion of this was absorbed by the sediments which now constitute bituminous strata. For example, the black shales of the Ohio Devonian rocks are two hundred and fifty feet thick, and in them the bitumen is uniformly distributed throughout the whole mass. This distribution would imply that the bitumen was once in such a state of fluidity as to allow it to diffuse itself. In the cavities of the large *Septaria*, sometimes seen in these shales, I find among the crystals of calc spar globular masses of pure bitumen, showing that the bitumen was at least in a semi-fluid state. This bitumen originated in the shales. The diffusion of bitumen in the slates and shales often found lying directly upon seams of bituminous coal would indicate that it had been soaked up into the sediments (this process being doubtless often aided by pressure) while in a fluid or semi-fluid state. In a special study of the distribution of bitumen in the Paleozoic rocks of Ohio which I have undertaken, and from which I hope to derive important results relative to the origin of bitumen, both animal and vegetable, the depth under water or beneath sediments at which the process of bituminization took place and the diffusion of the bitumen in certain sediments and not in others,—I think I have already found facts enough to prove that the bitumen now disseminated through our shales, &c., must once have been in a condition of fluidity somewhat akin to that of petroleum.

Mr. Hunt, p. 522, speaks of the oil-producing corals of Bertie as being "surrounded by solid crystalline encrinal limestone which is free from oil," and of the "light-colored limestones above and below" as being "not only destitute of oil but impermeable to it." In these cases the petroleum appeared to be completely confined within limestone walls only to be revealed by the well or excavation. May we not ask whether, if the surroundings of this petroleum had originally been different, that is, had there been proper sediments with suitable submergence and pressure, would not the petroleum have been absorbed and helped to constitute bituminous strata? But can we follow this reasoning beyond this point, and infer that all the free petroleum distributed throughout our wide oil fields was produced at the time of the original bituminization of organic matter, and was preserved by the nature of its surroundings from being absorbed, and that subsequently more or less of this petroleum ascended from its places of birth to accumulate in such receptacles as the fissures of the third sand-rock of Oil Creek, Pa.? If such were the origin and history of all our petroleum it would be reasonable to suppose that much of it would still be found *in situ*, i. e., where it originated; but instead of this, all the oil I have ever seen, except very insignificant quantities in isolated cavities in fossiliferous limestones, has evidently strayed far from its place of origin. It is seldom, indeed, that we find any oil in juxtaposition with bituminous strata of any kind. It is more often found in fissures in sand-rocks, rocks in which no oil could ever have been generated, for whatever organic matter they might have contained was too much exposed to atmospheric oxygen to admit of the possibility of any bituminization. It is not only impossible that the oil could have originated in these sand-rocks, or in the arenaceous shales which underlie them in western Pennsylvania, but is most probable that the oil ascended from the still lower rocks in the form of vapor which condensed in the superior cavities. In other words, the oil which, according to the theory, was formed far below in the original bituminization of organic matter, must have undergone a process of distillation.

In favor of the other theory, that petroleum, as now generally found, is the product of a distillation of bituminous shales, &c., as suggested by Dr. Newberry and others, the following arguments may be urged: 1st. Oil may be artificially produced by distilling such shales and other bituminous materials. In all essential respects, the analogy between the natural and artificial oils is complete. 2d. The phenomena of oil and gas exhibited in our oil fields greatly resemble those observed in the artificial distillation of oil from bituminous materials. These phenomena include inflammable gases, naphthas, heavy oils, asphaltums, &c. 3d. It is believed that some petroleum has been actually produced in the earth by distillation. Dr. Newberry, in an article on "Rock Oils of Ohio," thinks he finds local proof of the distillation of the petroleum in the great bituminous springs of California, from Tertiary lignites, there being evidences of recent igneous action in the region. European geologists have attributed a similar origin to the petroleums of Italy. Of course, where igneous action is intense, all the bitumen would be entirely driven off. The same would be true where the action is considerable and long continued, as in the anthracite coal region of Pennsylvania where the coal has lost its bitumen, but no oil was formed, or, if formed, it was soon dissipated in gas. 4th. There is an abundance of oil-making material in the earth. The subterranean retort is largely charged. 5th. A comparatively low temperature is believed to be adequate to set free the oil vapors. 6th. By this theory there might be produced an almost indefinite quantity of petroleum, since bituminous strata are found widely distributed. In this way the existence of petroleum in so many different geographical districts may be readily explained; whereas, by the opposing theory, we are not certain that petroleum, as such, has been produced by the direct bituminization of organic matter, except in few strata and in very insignificant quantity. Finally, the agency which would volatilize the liquid bitumen, or petroleum formed by direct bituminization, and bring it up and distribute it through the present oil horizons would certainly be adequate to distill the bituminous shales, &c., and bring up the oil to the same elevations.

It may, however, be objected, that if this theory of distillation be true, we ought somewhere to find the residuum, or debituminized shales, &c., remaining after the oil had been extracted. Such discovery could not justly be expected in surface rocks, because, according to the theory, the heat agency would at best be small and could be scarcely felt near the surface. The question, then, would be reduced to this, viz: do the borings in deep wells ever show that the deep bituminous strata have lost any of their original and normal quantity of bitumen?

I will present one or two facts which may have some bearing upon this point. I am indebted to the courtesy of Mr. R. K. Randolph, superintendent of the Carlisle Oil Co., for a record of a well 860 feet deep bored by him near Petroleum, West Va. This well is near the center of the strata marked C in fig. 2. The top of the well is in the lower portion of the Coal-measures. At 170 feet below the surface, Mr. R. struck a series of sand-rocks which continued 419 feet. I cannot suppose otherwise than that these sand-rocks are the geological equivalents of the Waverly sandstones of Ohio. Below these he passed through 265 feet of what the record terms a "gray shale with much soot." The position of these shales would make them the equivalents of the black shales of the Ohio Devonian formation, which in Ohio are 250 feet thick. They evidently contain some light carbonaceous matter in the "soot," but the record calls them "*gray* shales," not black. Mr. R. is familiar with "black shale," for he passed through two seams of it in the first 56 feet of the well. Now have these deep shales, nearly 600 feet down and situated within the double dislocation of strata already described, lost a part of their bitumen and been changed from black to gray? Unfortunately, I have not been able to obtain any sample of the borings in this shale, they, with the "soot," having been washed away. Mr. R. is boring his well still deeper. Should he soon enter the equivalents of the Cliff limestone of the Ohio Reports, I shall then feel assured that he has already passed through the exact equivalents of the Ohio Black Shales and found them "gray." Of course, such facts are not conclusive as to any positive loss of bitumen, but they are not without significance. Should I find many similar cases where strata, which are highly bituminous at their outcrop, are found to contain little bitumen at great depths, and at the same time, the rocks above these buried strata containing in their fissures much oil, I think the inference, that the oil was derived from the bituminous shales, not unwarranted.

Marietta, O., March 20, 1866.

ART. V.—*Notes on Japanese Alloys;* by RAPHAEL PUMPELLY.

THE following notes, relating to the composition of some of the many alloys in use among the Japanese, are based on information obtained from native metal-workers. In a few instances, as with the *shakdo* and *gin shi bu ichi*, the process of manufacture, generally hidden, was shown me.

I. *Shakdo*, an interesting alloy of copper and gold, the latter metal in proportions varying between 1 p. c. and 10 p. c. Objects made from this composition, after being polished, are boiled in a solution of sulphate of copper, alum and verdigris, by which they receive a beautiful bluish-black color. I can explain this color only by supposing that the superficial removal of the copper exposes a thin film of gold, and that the blue color produced is in some manner due to the action of light on this film of gold.

The intensity of the color, and to a certain extent, the color itself, are proportionate to the amount of gold, one or two per cent of this metal producing only a rich bronze color. Pure copper treated in the above solution received the appearance of an enamelled surface with a rich reddish tint, and brass a similar surface with a darker shade. *Shakdo* is used for a great variety of ornaments, as sword-guards, pipes, clasps, etc.

II. *Gin shi bu ichi* ("quarter silver") is an alloy of copper and silver, in which the amount of silver varies between 30 and 50 per cent. Ornamental objects made from this composition take, when subjected to the action of the above solution, a rich gray color much liked by the Japanese. It is used for sword ornaments, pipes, and a great variety of objects.

III. *Mokume;* several alloys and metals of different colors associated in such a manner as to produce an ornamental effect. Beautiful damask work is produced by soldering together, one over the other in alternate order, thirty or forty sheets of gold, *shakdo*, silver, rose copper, and *gin shi bu ichi*, and then cutting deep into the thick plate thus formed with conical reamers, to produce concentric circles, and making troughs of triangular section to produce parallel, straight or contorted lines. The plate is then hammered out till the holes disappear, manufactured into the desired shape, scoured with ashes, polished, and boiled in the solution already mentioned. The boiling brings out the colors of the *shakdo*, *ginshibuichi*, and rose copper.

IV. *Brasses* (*Sin chu*).—The finest quality of brass is formed of 10 parts of copper and 5 of zinc. A lower quality, of 10 parts copper and 2·7 zinc.

V. *Kara kane* (bell-metal).—First quality—copper 10, tin 4, iron ½, zinc 1½.

Second quality—copper 10, tin 2½, lead 1⅓, zinc ½.

Third quality—copper 10, tin 3, lead 2, iron ½, zinc 1.

Fourth quality—copper 10, tin 2, lead 2.

In forming the bell-metals the copper is first melted and the other metals added in the order given above. The best small bells are made from the first quality. Large bells are generally made from the third quality. The *kara kane* has a wide range of use in Japan.

Solders.—For bell-metal—brass 20, copper 10, tin 15.

For brass—first quality brass 10, copper 1½, zinc 6.

For silver—silver 10, first quality brass 5 or 3.

For *gin shi bu ichi*—silver 10, first quality brass 5, zinc 3.
For *mokume*—silver 10, first quality brass 1½.
For *shakdo*—fine *shakdo* 3, zinc 10.
For tin—tin 10, lead 5.

Among the Japanese articles made of copper that find their way to this country, there are some with a bright red surface, which is often taken to be either a lacquer or an enamel. These objects are made of copper containing red oxyd through the entire mass, and after receiving the requisite form and a high polish, are boiled in the mixture mentioned above.

ART. VI.—*Notes on Tides at Tahiti, and Earthquake phenomena;* by Dr. C. F. WINSLOW. From a letter to one of the Editors, dated Munich, March 26, 1866.

I RECEIVED the American Journal of Science for March this morning, and have read with great interest the article on the Tides at Tahiti communicated by Prof. Bache, upon the observations of Capt. J. Rodgers.

When at Tahiti in 1844 I was immediately struck with the anomaly in the tidal phenomena. I observed the daily wave more or less regularly (but with the eye alone) on the shore and in certain inlets between Taunoa and Papiete, from the 14th of May to the 14th of June, the period of my stay there. Residing near the shore at Taunoa and walking daily to and from Papiete, and riding round the beach beyond point Venus, I constantly had opportunity for observation. The tide was low in the morning and highest from 12 to 2 o'clock, as a common observation. When on the reef off Taunoa, about the 10th of June, late in the afternoon (my notes are at home and I do not remember the exact day), the tide rose later and I was obliged to abandon my observations and collections on account of this unexpected circumstance. I remained on the reef until the sea swelled to a depth of eight or ten inches, as waves would strike the barrier and then flow strongly over it. My observations were never made with mathematical accuracy, but the latter fact accords with the imperfect records of the tide-guage established by Capt. Rodgers for the same month in 1858.

The remark of Capt. Rodgers, that "the range of the tides seems to be considerably less near the solstice, than they are near the equinox," will be found to hold as a constant truth; for upon this point I made inquiry and requested observations to be made, which were afterwards reported to me by the late Capt. John Hall, a Boston gentleman, who was for many years a merchant at Papiete. The results of the observations are that *the* *highest tides occur in December and January, and the lowest in June and July; and the general observation relative to diurnal tides at Tahiti was, that the moon exerted less control over tidal movements than in other latitudes or places in the Pacific Ocean, although similar, but less marked, anomalies existed to a noticeable extent in other more western groups.*

I was at the island of Toubooai six days early in May of the same year, during which I was making more or less observations upon the reefs and shores, both on the northern and southern sides of the island. The tides appeared to flow there with their usual regularity, and rose to a greater height than at Tahiti.

I have been prompted to communicate these facts because I have long considered the tidal phenomena at Tahiti as important to physical science as they are curious and anomalous; and when carefully observed and studied, I have no doubt they will greatly enlarge our general cosmical knowledge, and establish a more correct tidal theory than exists at present, notwithstanding the high utility and value of that we now have.

While upon this subject of tidal movements in the Pacific Ocean, I will take occasion to mention that during a long period of observation upon the coast of Peru with reference to earthquake phenomena, I found, not only the highest tides to prevail at Callao and Paita in December and January, but also a series of enormous waves or sea-swells to be thrown from time to time upon the coast, varying from twenty-four to seventy hours in continuance, accompanied by unusual heights of the tide during the same period, and, on the contrary also, I remarked that the ocean exhibited an unusual tranquillity during the months of June and July. These phenomena do not appear to be connected with great atmospheric storms, nor do they hold any special relation to the force of prevailing winds near or distant, so far as I have been able to ascertain; but they increase with, and accompany, the swelling of the tides, and occur generally, not always, about the full of the moon. They sometimes break suddenly upon the coast. *They are annual and constant in their periodicity.* During my researches in the old Spanish records for earthquake phenomena, I have found them spoken of in the past century, and that they have often made ravages upon the coast to a smaller or greater degree. That which overwhelmed Callao in 1746 invaded the coast with a front swell of 40 feet, *forty-one and a half hours after* the first earthquake had suddenly devastated Lima and Callao, and *seventeen and a half hours after comparative tranquillity of the earth had prevailed.* This terrible wave extended for hundreds of miles both north and south along the coast, and seemed to be an exceptional event in *intensity* although not wholly so in periodicity, it having occurred at 4 P. M., Oct. 30th. It was without doubt connected dynamically in some manner with the action of the internal forces which produced that series of earthquakes, one of the most violent within historic periods. A great oceanic movement was also observed upon the coast of Chili, at Talcahuana, at 6.30 o'clock, the same afternoon, without earthquakes. What is very extraordinary also, the waters of the Marañon were equally disturbed the same night on which the earthquake agitated the coast of the Pacific, as we learn by the letter of a Jesuit missionary located among the Indians east of the Andes. Without knowing what had happened at Lima, he writes as follows: "On the 28th Oct. (1746), apparently about midnight (for here we do not know exact time), a very strong earthquake occurred at this mission. I slept at the time in a ravine of the Marañon where nothing was perceived but great waves encountering from above and below, which threatened the canoes with injury; and as there was no wind, we do not understand the cause." ("Yo entonces dormi en una playa del Marañon, donde no senti otra cosa que unas grandes olas, encontradas de arriba y abajo que ponian en peligro las canoas; y por que no hubo viento no entendimos la causa." *P. Leonardo Dcublér, de la Compañia de Jesus Irimaguas*, Nov. 23, 1746.) This mission was "200 leagues from Lima." The convulsion which overthrew Callao and Lima occurred at 10.30 *o'clock* P. M. *on the 28th of Oct.*, "five and three-quarters hours before the full moon." Nothing unusual occurred in the appearance of the ocean until 4 *o'clock* P. M. *on the 30th*, when a mountain of water, represented in the old accounts to fill the horizon as high as the island of St. Lorenzo when first noticed, swelled in upon the land. [In connection with this record I will take occasion to correct an *error* which prevails in the books relative to "the submergence of the old city of Callao." By careful examinations of the entire locality and of the ancient maps and records, I can state definitively that *no subsidence* took place in the site of Callao during that or any earthquake, and that the red appearances visible in some places near the shores, and which have been heretofore supposed by superficial observers to be submerged brick-work of the ancient city, are only organic and zoophytic growths of a red color which have spread themselves over larger and smaller stones. This I have determined by some personal hazard and numerous explorations.]

The periodical swellings of the Pacific Ocean, to which I have referred above, rarely begin in a notable degree before November or extend beyond February, and they are very marked from Tumbez (3° S. lat.) to the Chincha islands (14° S. lat.), between which points I have had much opportunity to observe and inquire about them. From the synchronism of periodic intensity of these oceanic phenomena with the periodic intensity of earthquake (or plutonic) phenomena in that region of the globe, and from the synchronism of periodic intensity of tidal movements, as they have been observed on the shores of Tahiti and of Peru, with the periodic intensity of earthquake and volcanic (plutonic) movements throughout the surface of the *entire* planet, I have been compelled to believe *solar influence* to be the predominating element in causal action, and that the lunar connection with terrestrial phenomena is reactionary rather than direct. That the periodicity and intensity of manifestation of internal dynamic energy are connected with the position of the earth in its orbit, holding inverse and constant numerical relations to the length and sweep of the radius vector, as a general law, my observations and researches have established beyond contradiction or doubt. And I have the strongest reasons to believe that systematic observations upon the oceanic movements at Tahiti and other groups in the South Pacific, and upon the coasts of Peru and Ecuador, will lead to the discovery of most important data through which our present lunar theory will be greatly developed, and probably profoundly modified.

As facts accumulate in different departments of observation, it is most interesting to discover a convergence of all terrestrial phenomena toward a central and unique causal agency. The late Prof. Kriel of Austria made many observations showing a connection of earthquakes with terrestrial magnetism. Dr. Klugé of Saxony is showing some remarkable synchronisms of volcanic eruptions with solar spots and variations of the magnetic needle. In connection with these important inquiries, Prof. Lamont, the learned director of the astronomical and magnetical observatory at Munich, lately informed me that some years since, when instituting a series of magnetic observations in a subterranean observatory, he descended one morning to examine his instruments and to his astonishment found them all in the most tumultuous agitation. He had never previously seen them in any similar state. The observatory was too profound and remote from highways to be affected by vehicles or passing loads of iron. He noted the time of this anomaly (9^h 10^m A. M. April 18th, 1842), and thought nothing more about it, until some days afterwards he noticed the occurrence of a violent earthquake in the Grecian Archipelago, and on referring to his record, found the earthquake was transpiring at the same moment with the magnetic disturbance in his observatory. Prof. Colla at Parma, Italy, made a similar observation upon his magnetic instruments the same morning. The synchronism is curious and suggestive, to say the least; and when the intensity of these various phenomena, whether internal or external to the surface of the planet, involving the dynamical tension of the air, ocean, magnetism, electricity, and plutonic force, all vary by more or less positive ratios with *the length and sweep of the radius vector*, thus exhibiting constant relations of all tidal and periodic phenomena to the position of the planet in its orbit as it moves from aphelion to perihelion, we are compelled to look to the central body for the causal agency of every fluctuation of terrestrial force, however slight, great, or anomalous this may be.

In conclusion, I can but express my earnest hope that Prof. Bache will solicit and obtain from Congress the necessary authority and means to institute accurate and persistent observations upon tidal phenomena at Tahiti and other points in the *South Pacific Ocean*, as there are many reasons to believe that results deduced from researches made in that distant hemisphere (which is so differently-formed from our own) will do more to enlarge our knowledge of mundane and planetary physics, than those instituted upon the Atlantic shores.

ART. VII.—*Further Contributions to the History of Lime and Magnesia Salts.* I.—By T. STERRY HUNT, LL.D., F.R.S.

CONTENTS OF SECTIONS.—72-80, Review of previous investigations; 81-87, Hydrated double carbonates of lime and magnesia; 88-95, Supersaturated solutions of carbonates of lime and magnesia; 96-101, Supposed decomposition of gypsum by dolomite; 102-110, Artificial formation of dolomite; 111-112, Its occurrence in nature.

IN 1859 I published in this Journal, [2], xxviii, 170, 365, the results of a series of investigations on some of the more common salts of calcium and magnesium, in the course of which I pointed out some important and hitherto unknown reactions. The prominent part which these bases play in the chemistry of the earth gives them a great interest in a geological point of view, and has led me to farther inquiries in the same direction, the results of which it is proposed to set forth in the present paper. For the better understanding of what is to follow, I shall first give a brief analysis of the principal facts detailed in the paper already referred to, of which this may be looked upon as a continuation. It will therefore be convenient for the purpose of reference to number the sections from § 71, with which that paper concludes.

§ 72. In sections 1–5 it was shown that the gradual addition of a solution of bicarbonate of soda to water holding in solution chlorids of magnesium and calcium, first precipitates the whole of the latter element as carbonate of lime, with but one or two hundredths of adhering carbonate of magnesia, and there is thus obtained at length a solution holding only chlorids of sodium and magnesium, with a portion of bicarbonate of lime, which, by evaporation at ordinary temperatures, is deposited as a nearly pure carbonate. A similar separation of the two bases is obtained when dilute solutions of neutral carbonate are substituted for the bicarbonate of soda. These reactions are intelligible when it is considered that hydrous carbonate of magnesia at ordinary temperatures decomposes the soluble salts of lime with the separation of carbonate of lime. This is true not only for the sulphate and chlorid, but also for the bicarbonate of lime (§ 6).

§ 73. The solutions of recently precipitated carbonate of lime in the presence of an excess of carbonic acid and in water holding alkaline and earthy chlorids, were found to present some interesting peculiarities. By adding solutions of bicarbonate of soda to carbonated water holding chlorids of calcium and magnesium, supersaturated solutions, containing at the ordinary temperature and pressure from 3·4 to 4·1 grams of carbonate of lime to the liter were readily obtained. These, however, at the end of a few hours spontaneously deposited the greater part of the dissolved carbonate of lime as a crystalline precipitate, retaining of it in solution only about 0 8 gr. to the liter. From this it was evident that chlorids did not permanently augment the solubility of the carbonate, since pure water, although yielding in like manner supersaturated solutions of bicarbonate of lime, does not retain in permanent solution at the ordinary pressure more than 0·838–0·915 gr. of carbonate of lime as bicarbonate (§ 3–7). This solubility of the carbonate of lime will be farther discussed in § 88.

§ 74. According to Bischof a liter of water saturated with carbonic acid dissolves only 1·33 gr. of carbonate of magnesia; in presence of alkaline and earthy chlorids I obtained, however, permanent solutions holding not less than 21·0 gr. to the liter, thus confirming the previous results of Bineau (§ 8). In § 9 I have described the observations of this chemist and my own on the spontaneous decomposition of solutions of the sesquicarbonate of magnesia, which after a time let fall in close vessels a precipitate of hydrated monocarbonate.

§ 75. As shown above, the presence of earthy or alkaline chlorids does not augment the permanent solubility of carbonate of lime in water, and it is found that the solvent power of such a solution, aided by a current of carbonic acid is no greater for precipitated carbonate of lime than that of pure water under similar conditions, the amount dissolved being considerably less than $\frac{1}{1000}$th (§ 73).

It was, however, shown that the presence of sulphate of soda or of magnesia nearly doubles the capacity of carbonated water for dissolving carbonate of lime. Water holding either of these sulphates in solution (in the proportion of $\frac{1}{100}$th or even less) and impregnated with carbonic acid readily takes into permanent solution, at the ordinary temperature and pressure a quantity of pure carbonate of lime equal to from 1·56 to 1·82 and even 2·0 grs. to the liter.

§ 76. This increased solubility was explained by the fact of a double decomposition giving rise to bicarbonate of soda or of magnesia and sulphate of lime, of which latter salt the liquids thus obtained were nearly or quite saturated solutions. (One part of sulphate of lime, according to my determinations, requires for its solution 372 parts of water at 16° C.) By adding to these liquids an equal volume of alcohol the whole of the lime is precipitated in the form of gypsum, and the filtrate retains in solution bicarbonate of magnesia or of soda, according as one or the other sulphate was employed (§ 10–19).

§ 77. It was further found that when such a solution containing sulphate of lime and bicarbonate of magnesia was slowly evaporated at temperatures of from 30° to 70° C., the lime was deposited as crystalline gypsum, mixed with more or less carbonate, while the more soluble bicarbonate of magnesia was only separated at a later stage of the evaporation as a hydrous carbonate (§ 20–27). The presence of chlorids of sodium and magnesium does not prevent these reactions, but chlorid of calcium is of course incompatible with the existence either of bicarbonate of soda or bicarbonate of magnesia.

§ 78. It was evident that in this newly discovered reaction between solutions of bicarbonate of lime and sulphate of magnesia, together with the results of their spontaneous evaporation, we have an explanation of the origin of those numerous and extensive deposits of gypsum which are accompanied by carbonate of magnesia, generally in the form of dolomite or magnesian limestone. Before, however, inquiring into the conditions under which this double carbonate may be formed, some experiments were undertaken to determine the relative solubilities of carbonate of lime, dolomite and magnesite in dilute acetic acid at different temperatures. It was found that this reagent, first pointed out by Karsten, could be used in the proximate analysis of mixtures of magnesite, dolomite and calcite, and made to yield results of considerable accuracy. For this purpose I employed in the earlier experiments an acid containing fifteen per cent of glacial acetic acid, which was employed at a temperature of 0° C. From the experiments detailed in §28, 29, it was evident that, although dolomite was not quite insoluble in these conditions, such a dilute acid at 0° C., might be employed to separate dolomite from carbonate of lime, and at higher temperatures for its partial separation from magnesite.

At a later period in the investigations it was found that a liquid containing only three per cent of acetic acid attacks pure carbonate of lime with lively effervescence at 16° C., and even at 0° C., and that it is capable of being used with still greater advantage than a stronger acid for the investigation of these mixtures of carbonates. In some further experiments to be detailed below, the action of an acid thus diluted upon an excess of the mixed carbonates was made available for a process of fractional separation (§ 103).

§ 79. In § 30 the experiments of Bischof, showing the sparing solubility of dolomite in carbonic-acid water, were cited, and in farther illustration the following observation, since made, may be recorded: One gram of a very pure crystalline dolomite in extremely fine powder was suspended in little more than half a liter of water, which was then saturated with carbonic acid at the ordinary pressure, and the mixture digested for eighteen hours at about 18° C. with frequent agitation. At the end of this time the water held in solution an amount equal to 0·15 gr. to the litre of the two carbonates, in the proportion of carbonate of lime 57, carbonate of magnesia 43. In order to determine the influence of time and of a greater surface, two grams of this same dolomite were digested under similar conditions for five days, at the end of which time the amount of the double carbonate dissolved was equal to 0·39 grs. to the liter. In order to show the relative solubilities in carbonic-acid water, of dolomite in fine powder and pure precipitated carbonate of lime, a mixture of one gram of each was digested for eighteen hours with half a liter under the conditions just described, when there were found in solution carbonate of lime 0·380 and carbonate of magnesia 0·007, equal to 0·015 of dolomite; so that only about four parts of the latter were dissolved for ninety-six of carbonate of lime.

§ 80. The next point of interest in my previous paper was an inquiry into the conditions in which the double carbonate of lime and magnesia, known as dolomite, may be generated. Starting from the well known fact that gypsums are generally associated with dolomite, (although great deposits of dolomite are often without gypsum), and from the unfounded notion that such dolomites are formed by a process of alteration from previously deposited limestones, Haidinger had suggested that the origin of the carbonate of magnesia might be due to a reaction between dissolved sulphate of magnesia and carbonate of lime at elevated temperatures under pressure. This reaction was subsequently verified by von Morlot, who obtained in this way at 200° C. a mixture of sulphate of lime and carbonate of magnesia. As I have, however, shown in § 31, 32, the carbonate of magnesia thus produced does not combine with any excess of carbonate of lime present, but forms a crystalline and very insoluble magnesite, readily separated from the carbonate of lime by dilute acetic acid. In addition to the experiments there given in proof of this, another subsequent one may be cited, where a mixture of crystallized sulphate of magnesia with two equivalents of pure precipitated carbonate of lime was heated for some hours in a closed metal tube to 200° C. The decomposition of the sulphate of magnesia was complete, and the carbonate of lime removed from the mixture held only 0·7 per cent of carbonate of magnesia, while the residue contained, besides sulphate of lime, carbonate of magnesia with only 1·3 per cent of carbonate of lime.

Marignac had endeavored to form the double carbonate by heating in a similar manner solutions of magnesian chlorid with an excess of carbonate of lime. In this case, as I have shown, the decomposition, even after several hours at temperatures of 150°–290° C. is but very partial, while the product analyzed by dilute acetic acid was chiefly carbonate of lime, mechanically mingled with magnesite and a small but variable proportion of the double carbonate (§ 34–36). In both cases the carbonate of magnesia formed at a high temperature passes more or less completely into magnesite, which, as might be expected, and as I have shown (§ 37), evinces no disposition to form with the lime a double carbonate.

In subsequent experiments, however, it was shown that when the hydrous carbonate of magnesia, mingled with carbonate of lime is exposed to heat in presence of water, combination ensues, and the double carbonate dolomite is generated between 130° and 200° C., and probably at lower temperatures. These results are given, with many details, in § 39–42, and further experiments of the same nature will be found in § 103–109.

Hydrated double carbonates of lime and magnesia.

§ 81. The results noticed in the last section gave rise to further inquiries into the affinity between the carbonates of lime and magnesia, and to the discovery of some artificial hydrated compounds of the two. The numerous hydrated double carbonates studied by Deville were all compounds of the alkalies (potash or soda) with magnesia or a magnesian oxyd. In his beautiful memoir on these salts published in 1851 (An. Ch. et Phys., [3], xxxiii, 75–106), besides a series of double salts containing alkaline bicarbonates with nine equivalents of water, Deville has described numerous neutral double carbonates having the general formula C_2MMO_6, which are either anhydrous or combined with three, four, or ten atoms of water, HO; ($H=1, C=6, O=8$). In these salts, which are all crystalline, the first metal is either potassium or sodium, and the second is magnesium, nickel, cobalt or copper. With zinc the formula of the double carbonate is less simple than for the preceding, being, according to Deville, $3NaCO_3, 8ZnCO_3, 8HO$. The mode in which these salts are formed is instructive; a mixture of the precipitated carbonates with the alkaline bicarbonate or sesquicarbonate is transformed by a gentle heat into the crystalline double carbonates, and in the case of cobalt to a mixture of the 4-hydrated and 10-hydrated salts. A paste of magnesia alba and bicarbonate of soda with water was found to be slowly changed at a temperature of from 60°–70° C. into transparent crystals of the anhydrous double carbonate, which crystallizes in the hexagonal system and as remarked by Deville may be regarded as a soda-dolomite C_2NaMgO_6. I have already shown, § 38–40, that when this is heated to 200° C., with a solution of chlorid of calcium, the sodium is replaced by calcium, and dolomite is formed. It was with the anticipation that under conditions similar to those made use of by Deville, it might be possible to obtain double carbonates of lime and magnesia, that the following experiments, resulting in the production of hydrated carbonates, were undertaken.

§ 82. The first step was to procure a solution of the chlorids of calcium and magnesium in equivalent proportions, and for this purpose a crystalline dolomite from Galt, in western Canada, whose only impurity was a few thousandths of carbonate of iron was selected. This being dissolved in hot hydrochloric acid nearly to saturation, a little chlorine or chlorate of potash was added, and the digestion continued with an excess of the dolomite till the whole of the iron was precipitated, and a pure concentrated solution of the two chlorids in equivalent proportions was obtained.

§ 83. When the above solution is mixed with a slight excess of a solution of pure monocarbonate of soda and the resulting pasty mass heated to from 65° to 80° C., the precipitate is wholly changed in a few hours into a dense white granular matter, which, under the microscope, is seen to consist of pearly translucent globules, either single or aggregated. They are usually about $\frac{1}{2000}$th of an inch in diameter, and although most frequently spherical, sometimes present the form of disks having a radiated structure and ragged edges. Lobed and compound shapes from the coalescence of these disks are also met with. This substance is so slowly attacked by cold dilute acetic acid that it was at first mistaken by me for true dolomite, and described as such in a note to the American Philosophical Society before I had discovered water in its composition. It, however, gives off an abundance of water when heated in a glass tube, even after having been dried at 35° C. In the analysis of three several preparations of this compound, in which the lime and magnesia were calculated as neutral carbonates, there was always a deficit of from seven to nine per cent, which was regarded as altogether water. In a subsequent preparation, however, it was found that when freed by washing from all trace of chlorids it yielded a quantity of soda equal to 1·86 per cent of carbonate of soda, and moreover that there was a deficiency in the amount of carbonic acid, which was only about nine-tenths of that required to form neutral carbonates with the bases; so that the compound is a slightly basic carbonate of lime and magnesia with a little soda, and with about ten per cent of water. Further analyses are required of this substance, which appears to be nearly related to the native hydrodolomite or dolomite-sinter of Kobell.

§ 84. The magma obtained as in the last section slowly changes at ordinary temperatures into a crystalline compound much more highly hydrated than the last. A tall cylindrical jar filled with the paste of the freshly precipitated carbonates and exposed to the light at a temperature of 15° to 18° C., after twenty-four hours showed a layer of liquid at the surface from a partial subsiding of the precipitate, which, at the end of twelve days in one case, and twenty-five in another, occupied only one-seventh of the original volume. The process of change appeared to consist in the formation of nuclei, from which crystallization proceeded until every particle of the once voluminous, opaque and amorphous precipitate had become translucent, dense and crystalline. The supernatant liquid was alkaline from an excess of carbonate of soda, and held only traces of carbonate of magnesia in solution. The precipitate washed by decantation and dried on blotting paper, consisted of brilliant transparent, vitreous prisms, apparently oblique, grouped around centers, and sometimes forming little spheres five or six millimeters in diameter bristling with points. These crystals are permanent in closed vessels, but by exposure to the air slowly become opaque on the edges, without, however, losing their hardness. Heated in a glass tube they give off much water with decrepitation. The following are the results of two analyses of portions of this carbonate from the same preparation, but dried at different times by exposure to the air for several hours at about 18° C. The carbonates were supposed to be neutral though no determination of the carbonic acid was made. The carbonate of soda was separately determined on five grams, the absence of chlorids having been established, and the water estimated from the loss:

	I.	II.
Carbonate of lime, - -	37·74	36·98
" " magnesia, - -	31·38	31·06
" " soda, - -	2·18	2·18
Water, by difference, - -	28·70	29·78
	100·00	100·00

§ 85. Without farther analysis it would be hazardous to attempt to fix the composition of this double carbonate. The conditions under which it is generated are precisely those of the hydrated neutral carbonate of magnesia, and it is therefore not probable that there is any loss of carbonic acid. The carbonate of soda may exist as an integral part of the compound, or as an admixed double carbonate with lime like gaylussite. A partial loss of water in drying was evident from the aspect of the crystals, and the formula $C_2CaMgO_6, 5HO$, which requires carbonate of lime 36·5, carbonate of magnesia 30·7, water 32·8, may probably represent the true composition of this double carbonate. It is worthy of remark that the carbonate of lime and soda, gaylussite, has a similar formula, $C_2CaNaO_6, 5HO$, and that it may be artificially formed by a process which recalls that described above. According to Fritzsche when one volume of a solution of chlorid of calcium, density 1·15, is agitated with ten volumes of a solution of carbonate of soda, density 1·20, a gelatinous precipitate is formed which slowly changes into a deposit of monoclinic prisms of gaylussite, generally mixed with carbonate of lime. It was also found that a precipitate of pure carbonate of lime, after several days digestion with a concentrated solution of carbonate of soda was converted into the crystalline double salt, (Jour. fur prakt. Chimie, xciii, 339).

§ 86. In another experiment a portion of the solution of the chlorids of calcium and magnesium in equivalent portions, was by accident mixed with a quantity of carbonate of soda insufficient for its complete decomposition. The mixture was set aside and crystallized as before, but the process was slower, occupying five weeks. The liquid retained a portion of chlorid of magnesium, but only traces of carbonate of magnesia in solution. The solid product, like that described in § 84, consisted of transparent grains and groups of hard vitreous prismatic crystals. Dried in the air they became opaque but did not lose their hardness, and still gave off water with decrepitation when heated in a glass tube. Two analyses of this substance after several hours exposure to dry air, gave as follows; the bases being calculated as neutral carbonates and the water determined by the loss:

	I.	II.
Carbonate of lime, - -	51·30	50·52
" " magnesia, - -	29·97	30·09
Water, by difference, - -	18·73	19·39
	100·00	100.00

The ratios deduced from the above are nearly ten atoms of lime, seven atoms of magnesia, and twenty-one of water. From these results it is impossible to construct a simple formula, and lthough Deville has represented one of the neutral carbonates btained by him as containing three atoms of soda, eight of xyd of zinc, and eight of water, I should prefer, pending arther investigation, to regard the above described substance as mixture of two or more crystalline hydrocarbonates of lime nd magnesia. It is worthy of note that while the simple arbonate of magnesia retains in crystallizing 6HO, the compound of one equivalent each of lime and magnesia contains only 5HO, and the last, in which the lime predominates, is much less hydrated. These double carbonates deserve a more careful study than I have been able to give them, and I publish these incomplete observations in the hope that some one may extend my inquiries, and probably correct some of my determinations.

§ 87. The hydrous carbonate of magnesia, $C_2Mg_2O_6$, 6HO, is, as observed by Fritzsche and by Soubeiran, formed under conditions analogous to the double carbonates of lime and magnesia just described. The amorphous paste obtained by mixing a solution of sulphate of magnesia with a slight excess of carbonate of soda undergoes a change precisely similar to that of the mixed carbonates, and is transformed into small prisms aggregated into spherical masses, like the double carbonates of lime and magnesia. It is tolerably permanent in the air, and yielded me 29·0 per cent of magnesia; which exactly corresponds with the above formula.

Supersaturated solutions of carbonates of lime and magnesia.

§ 88. In § 73 allusion was made to previous experiments on the solubility of carbonate of lime in presence of an excess of carbonic acid. I found that by the addition of bicarbonate of soda to a solution holding chlorids of sodium, calcium and magnesium (with or without sulphate of soda), and saturated with carbonic acid, it is possible to obtain transparent solutions holding from 3·40 to 4·16 gr. of carbonate of lime to the liter. Of this, however, the greater part was deposited in the course of twenty-four hours, and the solution was then found to hold somewhat less than 1·0 gr. of the carbonate of lime in permanent solution with an excess of carbonic acid. Comparative experiments, moreover, showed that the presence of the chlorids above mentioned does not increase the amount of bicarbonate of lime which water is capable of holding *permanently* in solution; although, as pointed out in § 56 of my recent paper on Natural Waters (this Journal, [2], xl, 196), it would seem from the comparative experiments of Boutron and Boudet that these chlorids favor the formation of unstable supersaturated solutions.

§ 89. We have now to speak of supersaturated solutions of carbonates of lime and magnesia without any excess of carbonic acid, of which a brief notice is given in the section of that paper just cited. The power of alkaline chlorids and of chlorid of calcium to prevent the precipitation of carbonate of lime, and even to dissolve it when precipitated, has already been observed by Berthollet and by Storer (Dictionary of Solubilities, 110), but the inquiry does not appear to have been pursued farther. In like manner the power of salts of potash, soda or magnesia to prevent the precipitation of magnesia by alkaline carbonates, was noticed by H. Rose and by Longchamp (Gmelin's Handbook, iii, 225). These reactions I have made the subject of careful experiments, one object of which was to determine whether hydrated or anhydrous double carbonates of lime and magnesia might be obtained from such solutions of these carbonates containing no excess of carbonic acid.

For the following experiments there were employed solutions of pure neutral chlorid of calcium, and of pure calcined carbonate of soda, containing respectively 0·0555 gr. and 0·0530 gr. to the cubic centimeter, so that the mingling of this quantity of each would give rise to 0·050 gr. of carbonate of lime.

§ 90. I have found that the *recent* precipitate produced by a solution of carbonate of soda in chlorid of calcium is readily soluble in an excess of the latter salt or in a solution of sulphate of magnesia. The transparent, almost gelatinous, magma which results when solutions of carbonate of soda and chlorid of calcium are first mingled, is immediately dissolved by a solution of sulphate of magnesia, and by operating with solutions of known strength, as indicated above, it is easy to obtain transparent liquids holding in a liter, besides three or four hundredths of hydrated sulphate of magnesia, 0·8 gr. and even 1·2 gr. of carbonate of lime, together with 1·0 gr. of carbonate of magnesia, the only other substance present in the water being the chlorid of sodium equivalent to these carbonates. A solution of chlorid of magnesium, holding some chlorid of sodium and sulphate of magnesia, in like manner dissolved 1·0 gr. of carbonate of lime to the liter. Such solutions have an alkaline reaction.

§ 91. These solutions, which contained in all cases neutral carbonates with no excess of carbonic acid, possessed a considerable degree of stability. One prepared with 0·8 gr. of carbonate of lime and 1·0 gr. of carbonate of magnesia to the liter, was filtered after standing for eighteen hours at 16° C., and still retained in solution 0·72 gr. of carbonate of lime, which was, however, readily and completely precipitated by an equal volume of alcohol of 95 per cent. After a longer time, however, these solutions gradually deposit small transparent crystals of hydrous carbonate of lime (which also adheres as a crystalline powder to lines marked on the side of the vessel by a stirring-rod), and at the end of eight or ten days at the ordinary temperature the solution holds no more lime in solution, although still retaining all its carbonate of magnesia. When the recent solution is boiled there is formed a copious precipitate of carbonate of magnesia, with some lime, and after evaporation to dryness in a water-bath, a portion of soluble lime-salt remains in the residue.

§ 92. The transparent crystals which are slowly deposited from these solutions contain neither magnesia nor sulphuric acid. At low temperatures they are permanent in the air, but when heated to about 30° C. change into an opaque pasty mass. Analysis gives for their composition, carbonate of lime 52·3, water 47·7. These crystals agree in their physical characters with the decahydrated carbonate of lime, $C_2Ca_2O_6$, 10HO, which requires water 47·3.

§ 93. As shown in § 75, precipitated and even crystalline carbonate of lime is permanently soluble to a large extent in solutions of sulphate of soda or of magnesia in presence of an excess of carbonic acid, in which case sulphate of lime and bicarbonate of soda or of magnesia result from double decomposition. This process is, however, entirely different from the ready dissolving of recently precipitated and as yet unaggregated carbonate of lime in solutions of sulphate or chlorid of magnesium which contain no carbonic acid, and have no power to form permanent solutions of the carbonate of lime. The difference in the condition of the lime in the two cases is readily shown by the action of alcohol, which from the first solutions at once precipitates the whole of the lime as gypsum, and from the second separates it no less completely in the form of carbonate. It suffices, however, in the second case to saturate with carbonic acid before the addition of alcohol to reproduce the conditions of the first case, and obtain, instead of carbonate, sulphate of lime.

§ 94. The solubility of the yet uncondensed carbonate of lime in neutral solutions which are without action upon it in another state of aggregation, is a good example of the modified relations presented by bodies in the so-called nascent state, which probably in this case consists in a simpler and less condensed molecule. At the same time the gradual spontaneous decomposition of the solutions thus obtained affords an instructive instance of the influence of time on chemical changes.

§ 95. I have found that monocarbonate of magnesia is still more soluble than monocarbonate of lime under the conditions described, and have in this way obtained more than 5·0 gr. of carbonate of magnesia in solution in a liter of water holding six per cent of hydrated sulphate of magnesia and a little chlorid of sodium. This solution, strongly alkaline in its reactions, gave, when gently heated, a copious precipitate, which was almost wholly dissolved after some hours repose in the cold. I have already shown (§ 23) how obstinately sulphate of magnesia retains a portion of carbonate of magnesia even after the solution has been long boiled, or evaporated to dryness; this is manifested by an alkaline reaction and by the power of precipitating the nitrates of silver and copper. That these reactions are due to dissolved carbonate, and not to a sulphate with excess of base, is indicated from the fact that the addition of small quantities of hydrate of soda to a solution of sulphate of magnesia gives rise to a precipitate of hydrate of magnesia which is insoluble in the sulphate, and does not communicate to it an alkaline reaction.

On the supposed decomposition of gypsum by dolomite.

§ 96. Haidinger, according to Bischof, has endeavored to explain the origin of the sulphate of magnesia observed in many gypsum quarries by supposing a decomposition of the sulphate of lime by dolomite, and Suckow has also proposed the same explanation to account for an efflorescence of Epsom salt near Jena (Chem. Geology, i, 430, iii, 159). Bischof mentions in connection with the latter instance an observation of Mitscherlich, which from the omission of a few words in the English translation, seems to imply that this chemist had observed the complete decomposition of bitter-spar by a solution of gypsum. On reference to the German edition, however, it appears that Mitscherlich had observed the decomposition of carbonate of magnesia by gypsum, and thus that his experiments do not confirm the hypothesis of Haidinger and Suckow, which we have here to examine. It is therefore by an error that in my recent essay on Natural Waters, § 19–21, I have attributed the views of these geologists to Mitscherlich, whose original memoir cited by Bischof is not accessible to me.

§ 97. In the paper just cited I have recorded the following experiments: A solution of gypsum was made to percolate slowly through a column of several inches of finely powdered dolomite previously washed with pure water. After ten successive filtrations of the liquid, occupying as many days, no perceptible amount of sulphate of magnesia was formed. Solutions of gypsum, and others of chlorid of calcium, were then digested for several months at the ordinary temperature with pulverized dolomite, and also with native crystalline carbonate of magnesia (from Styria), with similar negative results. Solutions of gypsum impregnated with carbonic acid were also allowed to remain in contact with pulverized dolomite and magnesite during the warm season for a period of six months, and even then only traces of magnesia were taken into solution.

In one experiment out of many, 10·0 gr. of pure crystalline dolomite from Galt (§ 82) and 1·0 gr. of pure crystalline gypsum were digested with 200 c. c. of water at from 15°–18° C. for six days, when the filtrate, freed from gypsum by evaporation and the addition of alcohol, gave no trace of magnesia. The residue was then treated for the same time with water holding carbonic acid in solution, and the filtrate having been evaporated to dryness, gave to water an amount of sulphate corresponding to 0·010 of carbonate of magnesia for the 200 c. c., equal to $\frac{1}{1000}$th of the weight of the dolomite. The digestion of a similar mixture of dolomite and gypsum with pure water for six days at from 50° to 60° C., with frequent agitation, gave no appreciable amount of soluble magnesian salt. When, however, dolomite was digested at this temperature with a solution of chlorid of calcium for twenty-four hours, an amount of chlorid of magnesium equal to $\frac{2}{1000}$ths of the dolomite was formed.

It was evident from these and similar experiments that no reaction takes place between dolomite and solutions of gypsum even at 60° C., except in the presence of carbonic acid, whose solvent action on dolomite (§ 79) causes the formation of a small amount of sulphate of magnesia. It was then necessary to search elsewhere for an explanation of the origin of the magnesian sulphate found in the conditions observed by Haidinger and Suckow.

§ 98. The hydrous carbonates of magnesia are readily attacked by gypsum, a solution of which is soon decomposed when agitated with an excess of the sex-hydrated carbonate (§ 87) with the separation of the whole of the lime as carbonate. In like manner I have found that the white earthy hydrocarbonate of magnesia from Hoboken, N. J., gives rise to a large amount of sulphate of magnesia when digested for twelve hours in the cold with a solution of gypsum. This hydrated carbonate also completely decomposes protosulphate of iron in the cold.

§ 99. The absence of any hydrous carbonate of magnesia from the Galt dolomite was shown by its complete indifference to the action of gypsum solutions. It was, however, possible that some other dolomites might contain portions of such a carbonate intermixed. Accordingly a white earthy magnesian limestone from Chaumont,[1] belonging to the gypsiferous series of the Paris basin, was selected for experiment, pulverized, washed and dried. Of this 10 0 gr. were digested for six days with 1·0 gr. of gypsum and 250 c. c. of water at from 15°–18° C. At the end of this time sulphate of magnesia equal to 0 025 of carbonate was found in solution. The residue was then farther digested for the same time with 250 c. c. of a solution of gypsum saturated with carbonic acid, and gave a farther amount of sulphate equal to 0·052 gr. carbonate of magnesia. That the first action of the gypsum was upon a hydrous carbonate appears from the fact that the residue from the above processes, when farther digested for ten days in the cold with a fresh portion of pure gypsum solution and frequently agitated, gave only traces of sulphate of magnesia.

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[1] In a note published in 1860 in this Journal, [2], xxix, 284, I showed for the first time that the gypsum of the basin of Paris, France, is immediately overlaid by dolomite. It was there stated that two specimens of the so-called *white marls* collected by me at the gypsum quarries at Chaumont were found to contain about sixty per cent of magnesian carbonate of lime, mixed with clay. One of these specimens was traversed by veins of fibrous gypsum. As it was this dolomite which was used in the above experiment, the results of two analyses of it made at that time are here given. The rock is soft, white, earthy, somewhat conchoidal in fracture, and adheres strongly to the tongue. Hydrochloric acid attacks it but fee-

§ 100. It was, however, possible that besides hydrous carbonate of magnesia an admixture of hydrate of magnesia might also in some cases intervene to effect the decomposition of gypsum. The native crystalline hydrate, brucite, in presence of a solution of gypsum containing carbonic acid readily gives rise to sulphate of magnesia, and the rock known as predazzite is shown by the analyses of Roth and the subsequent ones of Damour to be an intimate mixture of carbonate of lime and a hydrate of magnesia similar to brucite. A portion of pulverized predazzite was found to evolve a strong odor of ammonia when digested with a cold solution of sal-ammoniac, which after seven days at 15° C. took up from the mineral 3·95 p. c. of magnesia and 0·30 p. c. of lime. 10·0 gr. of pulverized and carefully washed predazzite and 1·0 gr. of gypsum were digested for five days with 250 c. c. of water at 15°–18° C., and the liquid then

bly in the cold, and hot dilute acetic acid was employed for its analysis. The two specimens gave as follows:

	I.	II.	
Carbonate of lime,	36·4	36·7	= 59·3 p. c.
" magnesia,	25·9	25·2	= 40·7 "
Insoluble,	30·1	30·2	
Water, alumina and loss,	7·6	7·9	
	100·0	100·0	

The insoluble residue was a fine tenacious clay with a little sand. A portion separated from I. by acetic acid, and not ignited, was readily decomposed by sulphuric acid without any effervescence, and gave as follows: Silica 53·2, alumina and a little iron-oxyd 19·3, magnesia 6·4, lime a trace, water and loss 17·0, insoluble sand 4·1=100·0. This clay evidently includes, like some argillites which I have described in the Geology of Canada, page 601, a portion of a magnesian silicate, which may either exist as a double silicate with alumina analogous to chlorite, or as a simple hydrous silicate like the sepiolite or magnesian marl which is common in the Tertiary strata of many parts of Europe. (See my notice, this Journal, [2], xxxii, 286.) A specimen of this substance collected by myself near Paris in the lacustrine series known as the St Ouen limestone, which immediately underlies the gypsum, was thinly laminated and enclosed masses of menilite. It effervesced with cold acetic acid, which removed from it 9·0 per cent of carbonate of lime, and traces of magnesia. The residue was completely decomposed by heated sulphuric acid, which was blackened by a portion of organic matter present in the mineral. The analysis gave silica 58·4, magnesia 20·9, lime a trace, alumina and iron-oxyd 3·0, volatile 17·0=99·3. This silicate is readily decomposed by sulphuric acid, even after ignition. In my paper on Natural Waters, this Journal, [2], xl. 49, will be found some observations on the artificial formation of magnesian silicates, a subject which I propose soon to discuss in a separate paper.

ontained but a trace of magnesia in solution. To the residue as added 250 c. c. of a solution of gypsum partially saturated ith carbonic acid. After twenty-four hours digestion 200 c. c. f the liquid were found to yield on evaporation little gypsum, ut an amount of sulphate of magnesia equal to 0 330 gr. of arbonate. A second portion of gypsum solution with carbonic cid being added gave, after frequent agitation for seven days, a uantity equal to not less than 8·93 gr. of sulphate of magnesia to the liter, showing that a considerable portion of gypsum besides that first in solution took part in the reaction. In this, as in all the previous experiments, a coarsely crystalline and very pure gypsum, previously pulverized and washed with distilled water, was made use of.

§ 101. From all these experiments it appears that although dolomite has at ordinary temperatures no power of decomposing solutions of gypsum, this power is preserved by the native hydrocarbonate of magnesia, which seems to be present in small quantities in some magnesian limestones. It also appears that, with the intervention of carbonic acid, hydrate of magnesia, and rocks like predazzite containing this substance, may decompose solutions of gypsum with the formation of sulphate of magnesia. The native hydromagnesite, which may be represented as a compound of monocarbonate and hydrate of magnesia, is probably resolved by a solution of gypsum into carbonate of lime and hydrate of magnesia, a mixture like predazzite, which requires, as in § 99, the intervention of carbonic acid to enable it to decompose a further portion of gypsum.

Production of dolomite.

§ 102. In § 80 I have already discussed the conditions under which the anhydrous double carbonate of lime and magnesia may be formed, and referred to the experiments in a previous paper in which I had succeeded in producing it at temperatures considerably above 100° C. It was with a hope of obtaining it at lower temperatures that many of the experiments already detailed in this paper were undertaken. Thus it was not impossible that from the supersaturated solutions holding both the monocarbonate of lime and that of magnesia, a compound of the two might be deposited. The experiments already described, however, show that the carbonate of lime separates completely after a time as a hydrate, without any trace of carbonate of magnesia. Again it was hoped that the slow union of the two carbonates at temperatures below 100° C. might give rise to the anhydrous double salt, instead of which, however, we have seen that there are formed the hydrated double carbonates already described. Attempts were next made to dehydrate these compounds and thus produce dolomite, but with partial success.

§ 103. The following experiments were made in confirmation of those described in 1859. In the examination of the products obtained, acetic acid was made use of as before, but with modifications (§ 78). A dilute acid was prepared by mixing three measures of the glacial acid with ninety-seven of water. Of this liquid, containing three-hundredths of acetic acid, there would be required in round numbers about 44 c. c. for the solution of one gram of dolomite, upon which the action is comparatively slow at the ordinary temperature, although this same liquid dissolves carbonate of lime with lively effervescence. By dividing into two or more portions the amount of this dilute acid required to dissolve a given weight of a preparation of the two carbonates, and keeping separate the matters dissolved by the successive portions, a fractional analysis of the material is effected, which gives results still more satisfactory than those obtained by the method described in the previous paper.

§ 104. In § 37 it was shown the anhydrous crystalline carbonate of magnesia evinces no disposition to combine with carbonate of lime, and the following experiments will show that the crystalline sexhydrated carbonate of magnesia (§ 87) is but little disposed to combination. A portion of this compound was intimately mingled with an equivalent of precipitated carbonate of lime and one-fifth of an equivalent of bicarbonate of soda, which would at an elevated temperature furnish carbonic acid that might aid the reaction of the earthy carbonates. This mixture formed into a paste with water was heated in a closed tube for two hours from 120° to 130° C., and then to 180° C. After six hours the matter was removed, washed with water and treated with acetic acid of three per cent, which at 0° C. produced a lively effervescence. The portion thus dissolved consisted of carbonate of lime with but 3·3 p. c. of carbonate of magnesia, while the residue was slowly but completely soluble in hydrochloric acid, and was carbonate of magnesia with only 3·2 p. c. of carbonate of lime. From this it appears that a portion of the double carbonate is formed in this experiment and remains mingled with resulting magnesite. In another experiment, in which no bicarbonate of soda was added, the portion soluble in dilute acetic acid contained 90·3 of carbonate of lime and the residue only 6·8 p. c., the remainder being carbonate of magnesia. The result of these experiments, like that of von Morlot, is thus chiefly a mixture of carbonate of lime with magnesia.

In the above as in all the experiments at temperatures over 100° C., here described, I have made use of bronze tubes holding about 14 c.c., with screw-caps made tight by an interposed disk of lead, and heated in an oil-bath.

§ 105. It was next to be seen whether the hydrous double arbonates would yield dolomite by dehydration. For this purpose a portion of the hydrocarbonate formed at the ordinary emperature (§ 84) was slowly heated with water to 180° C. The coarsely granular residue was treated by successive portions of acetic acid of three per cent, by which it was at first readily attacked. The last or fourth part was not perceptibly acted upon by cold dilute hydrochloric acid, but required heat and long digestion with this acid to effect its solution. The composition of the successive portions was as follows:

		Calc. Carb.	Mag. Carb.
I.	21 parts,	90·36	9·64
II.	21 "	99·06	·94
III.	20 "	82·09	17·91
IV.	38 "	9·52	90·48

These results show that as in the preceding section but little dolomite is formed, though the proportion of lime which still remains in IV, indicates a certain amount of the double salt. The presence of nearly ten per cent of magnesian carbonate (in which form the dissolved base is calculated) in I, as contrasted with less than one per cent in II, seems to be due to a partial decomposition of the magnesian carbonate during its dehydration, involving a loss of carbonic acid and the formation of hydrate of magnesia. A similar result is seen in the second experiment of the last section, while in the previous experiment this was to a great extent prevented by the presence of bicarbonate of soda.

§ 106. A portion of the hydrocarbonate with excess of lime-salt, described in § 86, gave, when treated like the last, a somewhat larger admixture of dolomite, and when the hydrous double carbonate formed at 80°–90° C. was gradually heated with water to 180° C., the fractional analysis of the product showed that a large proportion of dolomite had been formed. The composition of the first and last portions was as follows, the second having been lost, and the third completely dissolved.

		Carb. Cal.	Carb. Mag.
I.	25 parts,	58·3	41·7
II.	43 "	undet.	undet.
III.	32 "	46·5	53·5

§ 107. In another trial with a granular double carbonate prepared at about 60° C., and then heated as before, the following results were obtained by fractional analysis, a residue of pure carbonate of magnesia insoluble in acetic acid remaining.

	Calc. Carb.	Mag. Carb.
I.	66·7	33·3
II.	55·4	44·6
III.	00·0	100·0

It suffices to compare the last two results with those obtained in the previous two sections and those recorded in § 80, to see that in the present case a double anhydrous carbonate is actually generated. While in the previous preparations with sulphate of magnesia and carbonate of lime, or with the more hydrated double carbonate, the separation of the two carbonates by dilute acid is nearly complete, a large amount of carbonate of magnesia is in this case dissolved with the first portion, and the residue is, by the farther action of the dilute acetic acid, shown to be a mixture of dolomite with magnesite.

§ 108. But the most favorable conditions for the artificial production of dolomite, so far as yet observed, are attained with an intimate mixture of the two carbonates in the amorphous state, as precipitated by a slight excess of carbonate of soda from the solution of equivalent proportions of the chlorids of calcium and magnesium. (§ 82). To effect the union of the two carbonates the heat should be very gradually raised to 120°–130°, and there maintained for an hour or two. The following are results obtained from preparations thus obtained and submitted to fractional analysis by means of acetic acid of three per cent.

A precipitate of the two carbonates was made as above, pressed in linen, and the unwashed pasty mass gradually heated, thirty minutes being taken to raise the temperature from 100° to 140° C. Of the washed and dried matter the first thirty-four per cent contained 56·6, and the last ten per cent 53·0, of carbonate of lime. Another and partially washed precipitate of equivalents of the two carbonates, which had been dried at the ordinary temperature, and again moistened with water and heated to 170° C., was treated with successive portions of acetic acid with similar results to the preceding. The last residue of twenty-one per cent consisted of carbonate of lime 52·7, carbonate of magnesia 47·3.

It is unnecessary to multiply the descriptions of results of this kind obtained from five or six different preparations, and all showing that under the influence of heat the pasty mixture of the two carbonates yields an anhydrous, sparingly soluble compound having the chemical character and composition of dolomite, which requires carbonate of lime 54·35, carbonate of magnesia 45·65.

§ 109. In another experiment a mixture containing more than an equivalent of magnesian carbonate was heated as above described, and the portion dissolved by the first action of the acid contained 48·6 per cent of carbonate of magnesia, while the second portion dissolved had only 47·0 per cent, and the residue was pure magnesite. The excess of magnesia in the first fraction over the second would seem to be due, as in § 105, to a partial decomposition of the excess of hydrated magnesian carbonate in the mixture.

§ 110. Carbonic-acid water may be employed instead of acetic acid as a solvent for the analysis of artificial dolomite (§ 79). In a preparation of dolomite made in the way just described, and containing an excess of carbonate of magnesia, (52·0 per cent), the action of 500 c. c. of water saturated with carbonic acid during two and a half hours, removed from one gram 0·453 gr. containing only 48·5 per cent of magnesian carbonate. The residue, from which the more finely divided portions had thus been removed, was very slowly attacked by a solution of carbonic acid, a second portion of 500 c. c. of which, after four hours, took up 0·145, and a third portion, after eighteen hours more, 0·162 gr. of the two carbonates, in both cases consisting of carbonate of lime 53·0, carbonate of magnesia 47·0.

§ 111. In concluding this part of the subject it is to be remarked that two things in the history of dolomite may be regarded as established: first, its origin in nature by direct sedimentation, and not by the alteration of non-magnesian limestones; and second, its artificial production by the direct union of mixtures of the carbonates of lime and magnesia at temperatures above 120° C. The question next arises whether all dolomite strata have been exposed to such a temperature, or whether there are yet unknown conditions under which the double carbonate can be found at lower temperatures.

The magnesian limestone from the elevated coral island of Matea, described by Dana (this Jour., [2], xiv, 82), is, according to the analysis of Silliman, and my own subsequent examination and analysis (Ibid., [2], xix, 429), a true dolomite with a slight excess of carbonate of lime, and is regarded by Dana as of recent origin, and as derived, in some way, from the alteration of coral mud. If this origin be established beyond a doubt, it is to be remarked that the separation of carbonate of magnesia from sea-water requires peculiar conditions, which evidently are rarely fulfilled in the case of these coral deposits; and its production being conceded, the volcanic agencies so active in these regions may have very well furnished the heat requisite to form dolomite before the elevation of the island.

§ 112. Apart from the formation of stratified sedimentary dolomite, we have also to keep in mind the frequent occurrence of this double carbonate as a mineral of secondary deposition, lining drusy cavities, filling veins, and even the moulds of fossil shells (§ 52, 53). The conditions of its deposition from natural waters are probably not unlike those of the quartz, fluor, and heavy-spar, with which, in its form of bitter-spar, it is often associated, and as subjects for farther investigation, may yet throw more light on the agencies which have effected the union and crystallization of the two carbonates in sedimentary deposits.

Montreal, Jan. 1866.

Art. VIII.—*Remarks on the new division of the Eocene, or Shell Bluff Group, proposed by Mr. Conrad;* by Eug. W. Hilgard, Ph.D., State Geologist of Mississippi.

In a brief paper published in the January number of this Journal, Mr. Conrad proposes to distinguish, as a separate group of the American Eocene, a series of deposits but feebly represented at Vicksburg by a five-foot stratum of dark lignitic clay and sand, differing in its paleontological characters from both the Vicksburg and Jackson group. Mr. Conrad considers it to be especially characterized by the occurrence of *Ostrea Georgiana*, and defines it as *underlying* the "Orbitolite limestone of the Jackson Group." He also mentions, in the section of the Vicksburg Bluff, the Orbitolite limestone, as a representative of the Jackson group.

The latter supposition is manifestly an oversight on the part of my honored friend. That the group of fossils described by him, and figured in Prof. Wailes's Report, as Jackson fossils, do not *occur* at Vicksburg, I need not recall to his mind; but he has overlooked the fact that the *Orbitoides Mantelli*, throughout the state of Mississippi, at least, is entirely absent from the Jackson Group, the Orbitoides limestone being invariably accompanied by *Pecten Poulsoni*, *Arca Mississippiensis*, *Ostrea Vicksburgensis*, and other leading Vicksburg fossils.

Of *Ostrea Georgiana* I have unfortunately never seen an authentic specimen or description; but from the facts stated by Mr. Conrad, and his comparing it to *P. longirostris* Lamk., I unhesitatingly identify it with specimens from Vicksburg, labeled "*P. gigantea*" by Prof. Wailes. Upon the authority of the latter observer, Mr. Conrad mentions the occurrence of *O. Georgiana* at Jackson. There is, indeed, some resemblance between the lower valve of the oyster so abundant at Jackson (which, together with the bones of the Zeuglodon, characterizes the "shell prairies" of central Mississippi, as stated p. 128 of my Report, and some specimens of *O. Georgiana;* but in their general character, when seen in *series*, they differ widely, the Jackson oyster having distinctly the habitus of a *Gryphœa*, and oftentimes resembling closely *G. convexa* of the Rotten limestone. It is one of the leading fossils of what I have most unequivocally recognized as the upper member of the Jackson group; it occurs at Jackson itself, on the hill-tops, associated with *Zeuglodon* bones, *Umbrella planulata*, *Cypræa fenestralis*, *Morio Petersoni*, *Conus tortilis*, and others, in stratum No. 7 of section 27, page 131 of my Report. The Jackson fossils described by Mr. Conrad are derived from Nos. 4 and 5 of that section.

In the numerous localities where I have studied the beds of the Jackson group, I have never found a single Orbitoides associated with them. The constant concomitant of the latter fossil, the *Pecten Poulsoni*, also is absent from the Jackson strata, being replaced by *P. nuperus*, Con.

But if the Orbitoides limestone is no member of the Jackson, but on the contrary, a characteristic one of the Vicksburg group, then it is clear that the strata of the "Shell Bluff group" at Vicksburg lie *above*, and not below the Jackson strata. For it cannot be supposed that the latter, which occupy so extensive an area above Vicksburg (see the map accompanying my Report,) should suddenly come to an end, and leave no trace of a representative between the Shell Bluff and the Vicksburg groups did it belong there.

There is only one other locality in the state, as far as known, where *O. Georgiana* (i. e. the large oyster occurring at Vicksburg) is found, viz: in Jasper county, Miss., where it was collected by Prof. W. D. Moore, late of the University of Miss. It there occurs again in the same outcrop with *Pecten Poulsoni*, *Orbitoides*, and a *Schizaster*, which is also a leading Vicksburg fossil; this locality being, likewise, considerably south of the shell prairies of the Jackson group.

As there is nothing to justify the assumption of a sudden termination of the strata of the latter group, which, on the contrary, may be seen disappearing under those forming the transition to the Vicksburg strata, with remarkable regularity, along the course of both Pearl and Chickasawhay rivers, (see p. 135 of my Report), the conclusion is inevitable that *the Jackson group is older than the Shell Bluff group* as defined by Conrad.

That there may be a considerable difference in the geological horizons of the Jackson and Vicksburg groups proper, sufficient to admit of the existence of a fauna deserving to be formed into a distinct group, is proved, not only by the paucity of coincident species, (see list, ibid, p. 132), but no less by the considerable thickness of the intervening strata in eastern Mississippi, on the Chickasawhay river, which near Red Bluff Station (ibid, p. 135,) amounts to over one hundred feet.

Here, as at Vicksburg, we have, underlying the Orbitoides, marls and limestones, a stratum of inconsiderable thickness, but literally teeming with shells, which are a strange mixture of the faunas of Jackson and Vicksburg, with numerous peculiar species (see list, ibid, p. 136). Here also, we have a *Madrepora*, distinct from, but closely allied to, the species found in the "Georgiana bed" at Vicksburg; where in its turn we find an extraordinary number of valves of *Meretrix sobrina*, a *rara avis* in the Vicksburg strata proper, but abundant in the Jackson group. *Busycon undulatum*, also, is a Jackson form, if not species. The number of species thus far found in this bed is, however, too small to allow us to expect numerous coincidences either way. *Ostrea Georgiana* does not occur at Red Bluff, the greater part of whose strata, immediately superimposed upon the Zeuglodon beds, are extremely poor in fossils.

Of course, these data are insufficient as yet to parallelize Mr. Conrad's Shell Bluff and my Red Bluff group. But their relative positions seem to be at least analogous, and I sincerely hope Mr. Conrad will, before many months, give both the collections of the Mississippi Survey, and the localities themselves, the benefit of his personal inspection.

I should add, in conclusion, that the Jackson and Vicksburg groups are by no means always thus separated by intervening beds. In one locality, at least, at the extreme southern edge of the marine Tertiary, I have seen the white Orbitoides marl directly superimposed upon a bed of blue marl containing *Monoceros vetustus* and *Morio Petersoni*, with a *Rostellaria* thus far called *R. velata*, but which is certainly a species distinct from the Claiborne fossil of that name.

ART. IX.—*Preliminary Notice of certain beds of Fish-remains, in the Hamilton group of Western New York;* by FRANK H. BRADLEY.

ACCOMPANYING the Moscow black shale of the upper part of the Hamliton group, at Geneseo and Moscow, Livingston county, and East Bethany, Orleans county, N. Y., are certain thin lenticular masses of impure pyrites, which contain large quantities of the teeth, fin-spines and bony-scales, of fishes, and numerous Mollusca.

The layers composing these beds are very variable in thickness and in composition, some being quite solid and composed almost entirely of pyrites; others, thin and fragile, and interlaminated with layers of black shale. The latter portions commonly contain the bones, while the more solid portions yield shells most numerously.

It would seem that the sulphur of the pyrites must have come from the decomposed fish, and that the beds correspond to the deposits of fish-remains reported by dredgers in certain seas, while the surrounding bottom yields not a fragment.

Information concerning the situation of these localities was given by Mr. H. A. Green in the January number of this Journal.

So far as I have been able to ascertain, they had not been explored by any one previous to my visit in July, 1864, at which time, as well as in the succeeding year, I collected largely therefrom for the cabinet of Yale College. I have also employed Mr. Green to increase my collections. The specimens thus obtained are sufficient to indicate the existence of at least two or three species of fish, and to show the principal characters of one of them. It is hoped that further explorations, now in progress, will be still more successful.

The most common species has a two- or three-forked tooth, somewhat resembling that of *Diplodus*. The scales are all pustulose: some are rectangular, measuring one to two inches on a side; others are curvilinear, one of which measures six inches across (probably a distinct species). One large, probably interior, bone measures three by four inches, with a thickness, at one end, of over an inch. One jaw is between three and four inches long.

These remains all retain their bony structure, though some of the larger and more porous fragments are thoroughly permeated with the pyrites.

Accompanying these remains are very numerous shells of Cephalopods, Gasteropods, Brachiopods, and Lamellibranchiates, which are mostly replaced with nearly pure pyrites, and separate readily from the rock, with very brilliant surfaces.

A few of the Orthocerata retain their calcareous structure, and also have their cavities mostly filled with calcite. The same mineral is sometimes found in the interior of the Goniatites which are common in the beds.

Of *Goniutites*, there are at least three species, besides a very minute form which I am inclined to call the young of *G. uniangularis*, but which may prove distinct. Of Gasteropods, we have twelve or fifteen species; probably as many Lamellibranchs; and five or six Brachiopods. Excepting the Goniatites, which are sometimes two inches across, the shells are all minute.

A few specimens of three or four small species of *Pentremites* have been found, and the stems of larger crinoids are quite common in some layers. Corals are very rare.

Many of the species will very probably prove identical with those which crowd the Hamilton blue shales, but I have reason to think that most of them are new. No careful examination and comparison have as yet been made; but I hope that it may, ere long, be completed, and the results published.

In this connection, the following fact may be interesting:

From the *Black slate* of the Hamilton group at the West, which has proved almost entirely barren wherever worked, a German clergyman residing at Delaware, Ohio, has recently obtained, at that locality, nearly the whole of a thick, heavy pustulose scale, measuring about ten inches by fourteen; the pustules are about one-fourth of an inch in diameter. Considerable digging was done, but no further discoveries made.

Being absent from home, I have not the opportunity of referring to my specimens, and I cannot therefore make the present notice more complete.

Panama, U. S. C., May 11, 1866.

ART. X.—*On the Ammonium Amalgam;* by F. S. PFEIL[1] and HENRY LEFFMAN.

FOR some years the attention of chemists has been directed to the investigation of the substitution ammoniums. Notwithstanding their close analogy to ammonium itself, in many respects, we have not been able to find record of any systematic attempt to form amalgams analogous to the well known ammonium amalgam. The consideration of this fact induced us to commence a series of experiments to determine the deportment of these bodies with sodium amalgam.

A saturated solution of chlorid of trimethyl-ammonium was treated with sodium amalgam, and a series of phenomena followed exactly identical with those which occur in the preparation of the ammonium amalgam. The swelling rapidly subsided, hydrogen gas being given off, and the liquid was found to contain trimethylamine.

Saturated solutions of the chlorohydrates of aniline, conine, morphine and quinine, and of the acetate of rosaniline, when treated with sodium amalgam, give rise to copious evolution of hydrogen gas, without turgescence.

These experiments (in addition to those recorded by Dr. C. Wetherill) seem to indicate that the physical phenomena of the ammonium amalgam depend entirely upon the retention of gas bubbles, and also that those ammonias which, in the free state are, at ordinary temperatures, either liquid or solid, produce no amalgam.

It may be mentioned that a solution of chlorid of ammonium in pure glycerine gives rise to an amalgam, but the turgescence is much interfered with by the viscosity of the solvent; and also that sodium amalgam, when placed upon a crystal of chlorid of ammonium, produced no reaction until moistened with a drop of water.

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ART. XI.—*On Danalite, a new Mineral Species from the Granite of Rockport, Mass.;* by JOSIAH P. COOKE, Jr.

DISSEMINATED through the Rockport granite, which is quarried at the extremity of Cape Ann, Massachusetts, and much used for building in Boston and the vicinity, are occasional grains of a flesh-red mineral somewhat resembling Rhodonite.

The mineral has been at times found in masses of considerable size, and for a specimen of this sort I am indebted to the kindness of Mr. W. J. Knowlton, of the Lawrence Scientific School.

The characters of the mineral are as follows: Color, flesh-red to gray. Streak similar in color to the mineral but lighter. Lustre, vitreo-resinous. Translucent. Fracture subconchoidal uneven. Brittle. Hardness 5·5 to 6. Specific gravity—two determinations—3·427. The exterior portion of the mass showed no indication of crystalline form and there was no distinct cleavage; but on breaking it open a well developed octahedron of the regular system was found in the interior. The angle between the octahedral faces measured with an application goniometer 109° 30′. The edges of the octahedron were replaced by planes of a rhombic dodecahedron, strongly striated parallel to the longer diagonal of the face. The mineral, therefore, crystallizes in the *holohedral* forms of the monometric system.

Before the blowpipe the mineral readily fuses on the edges to a black enamel. Hence its fusibility is about 4 of von Kobell's scale. On charcoal with carbonate of soda it gives a slight coating of oxyd of zinc. In a closed tube it loses color, but gives off no water or any sublimate. It is perfectly decomposed after some time by hydrochloric acid, the silica partly gelatinizing. It is also decomposed by nitric acid; but then the silica separates as a powder. It is partially decomposed by dilute sulphuric acid, and even by acetic acid, sulphid of hydrogen gas being evolved.

In order to thoroughly decompose the mineral the material was finely pulverized and sealed up with some concentrated acid in a glass flask, which was then exposed for several hours to the heat of a water-bath. When hydrochloric acid was used a slightly greenish solution was obtained, frequently depositing crystals of protochlorid of iron on cooling, but showing no traces of sesquichlorid, and on opening the flask a strong odor of sulphid of hydrogen was observed. When nitric acid was used the flask became filled with nitrous vapors, and both the iron and the sulphur were completely oxydized. A qualitative analysis proved the mineral to be a compound of silica, glucina, protoxyd of iron, oxyd of manganese, and oxyd of zinc, mixed with the sulphids of the last three metals. The presence of alumina could not with certainty be detected by any known tests. The precipitate of glucina perfectly redissolved in an excess of carbonate of ammonia, and no crystals of alum could be obtained from a solution of the sulphate when treated in the usual way with an excess of sulphate of potash, although they were sought for with a microscope.

As the sulphid of hydrogen which is evolved from the metallic sulphids, when the mineral is decomposed by hydrochloric acid in a closed flask, would necessarily reduce all the iron present to the condition of proto-chlorid, the following experiment was made to determine the original condition of the iron in the mineral. It is evident that any such reduction must be attended with the separation of free sulphur, and hence sulphur was sought for in the products remaining in the flask after the decomposition was finished. The sulphid of hydrogen and the greater part of the free hydrochloric acid having been first expelled, the residue was boiled with an excess of concentrated nitric acid, and as no trace of sulphuric acid was found it was concluded that the iron in the mineral, not united with sulphur, was all in the condition of protoxyd. The same experiment also proved that none of the varieties of iron pyrites could be present in the mineral in distinct grains, as was at first suspected; and this conclusion was confirmed by the fact that a powerful magnet failed to attract any portion of the mineral, even when reduced to the finest powder.

In the quantitative analysis no unusual methods were employed. The mineral was decomposed in a sealed flask as described above, sometimes by hydrochloric but usually by concentrated nitric acid. The silica was separated in the ordinary way and its purity tested by hydrofluoric acid, the small amount of residue, which was in some cases obtained, being carefully determined and estimated in calculating the final result. The iron was then precipitated as basic acetate, and this precipitate carried down with it a portion of the glucina. From the filtrate the manganese was separated by bromine, and the zinc was precipitated by sulphid of hydrogen. The filtrate from the sulphid of zinc was always found to contain the larger part of the glucina,[1] which was precipitated by ammonia. The glucina thus precipitated was weighed down by itself and the amount added to that subsequently separated from the iron. This last separation was effected by a simple modification of Deville's method which will be described at the end of this article. Lastly, the sulphur, which now remained in solution as sulphuric acid, was determined as sulphate of baryta in the usual way. The results of my analyses were as follows:

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[1] We have never succeeded in precipitating the whole of the glucina as basic acetate, although carefully attending to all the precautions which have been indicated by other analysts. But alumina is perfectly precipitated when the necessary precautions are observed.

	1.	2.	3.	4.	Mean.
Silica, - -	31·74	31·54	31·96	31·69	31·73
Protoxyd of iron, -	....	....	25·71	29·09	27·40
Oxyd of zinc, -	17·90	16·90	19·11	16·14	17·51
Oxyd of manganese, -	5·83	6·64	6·17	6·47	6·28
Glucina, - -	....	....	13·86	13·79	13·83
Sulphur, - - -	....	....	5·93	5·02	5·48
			102·74	102·20	102·23
Oxygen equivalent to per cent of sulphur,			2·96	2·51	2·74
			99·78	99·69	99·49

For analysis 4 the material used was a portion of the crystal described above. When in mass it had a bright flesh-red color, and even in the powder the color was still quite decided. The material used in analysis 3 was taken from a wholly different portion of the specimen. The red tint was less marked and the color of the powder was decidedly gray. These two portions were selected as presenting the most decided contrasts of color observed. Analyses 1 and 2 were made before the composition of the mineral was correctly known, and the best method of analysis discovered. Hence, only a portion of the bases were accurately determined, and only those results are given which are known to be trustworthy. The material for all the analyses was selected with great care; but that used in 4 being a portion of a crystal from the center of a large mass was unquestionably the most pure.

The most natural theory of the constitution of the mineral, to which the above results and the crystalline form both point, is that the mineral is an isomorphous mixture of a monometric silicate with the simple sulphids of iron, zinc, and perhaps of manganese, all of which affect the same crystalline form. These different sulphids must be present in somewhat varying proportions; for while in 4 the sulphid of iron is evidently in excess, the sulphid of zinc equally predominates in 3, and such a difference is plainly indicated by the difference of color already mentioned. This view is also sustained by the action of different acids on the pulverized mineral. Dilute sulphuric acid attacks the powder even when cold, sulphid of hydrogen being evolved, while iron and zinc in large quantities, with some glucina, enter into solution. Even dilute acetic acid causes an evolution of sulphid of hydrogen dissolving an abundance of iron, but also some glucina. Since now sulphid of zinc is insoluble in acetic acid this last reaction points clearly to the presence of sulphid of iron in the mineral; but at the same time both experiments indicate that the metallic sulphids are so intimately associated with the silicate that the decomposition of the first involves to a limited extent at least the breaking up of the last. This is what we should naturally expect in an isomorphous mixture, the sulphids not being present in separable grains; but diffused through the mineral in a state of imperfect chemical combination, and thus rendering even a firm silicate exceedingly susceptible of decomposition.

On examining the results of analysis given above it will further appear, in support of the same theory of the constitution of the mineral, that while the proportions of silica, glucina, and even of oxyd of manganese, are very constant, the proportions of the oxyds of zinc and iron vary very considerably; these metals, although in the analyses separated and determined as oxyds, being, in fact, combined to a greater or less extent with sulphur in the different portions of the mass examined. If now we calculate on this view the oxygen ratio of the silicate, deducting of course from the sum of the oxygen of the bases, an amount of oxygen equivalent in each case to the per cent of sulphur found, it appears, taking the mean result, that the sum of the oxygen of the bases is to the sum of the oxygen of the silica as 16·93 : 16·81, or very nearly as 1 : 1. Moreover, if we regard glucina as a sesquioxyd base we shall have for the oxygen ratio between protoxyds, sesquioxyds and silica, the proportion 8·22 : 8·71 : 16·81 or very nearly as 1 : 1 : 2. Again, the amount of sulphur in the metallic sulphids is very nearly one-sixth of the amount of oxygen in the silicate; so that for every twelve equivalents of oxygen in the silicate we have one equivalent of sulphur in the sulphids. Hence we deduce as the general formula of the mineral

$$(\tfrac{1}{2}\dot{R}_3 . \tfrac{1}{2}\dddot{B}e)\dddot{S}i + \tfrac{1}{2}\dot{R}S \text{ in which } R = Fe . Zn . Mn.$$

The oxygen ratio of the new mineral is the type ratio of the garnet family, and to this family it undoubtedly belongs. Its mineralogical characters would place it intermediate between Willemite and Iron Garnet where by its composition it naturally falls. This relationship appears in the following table; but it is seen much more clearly on comparing the actual specimens:

	WILLEMITE.	DANALITE.	IRON GARNET.
Action of hydrochloric acid,	Gelatinizes easily.	Gelatinizes, but less easily and perfectly.	Decomposed, but does not gelatinize.
Before blowpipe,	Fuses on edges to white enamel.	Fuses more readily to black enamel.	Fuses easily to a bead.
Sp. gr.,	3·89 to 4.	3·427.	3·7 to 4·2.
Hardness,	5·5	5·5 to 6.	6·5 to 7·5.
Luster,	Vitreo-resinous, weak.	ditto, stronger.	ditto, brilliant.

It is true that Willemite and Garnet belong to different crystalline systems, but the ordinary form of Willemite really ap-

aches very nearly a rhombic dodecahedron, the difference
angle only amounting to 5°, and there is no good reason for
aching a greater significance to this difference of angle
ween the corresponding forms of allied systems, than is
ached to an equal difference between similar forms of the
ne system. In its peculiar constitution the new mineral is
ied to Helvin, although the two species have no outward re-
nblance. The holohedral character of its crystallization, the
y large amount of iron and zinc entering into its composition,
color, luster, hardness, and other physical as well as chemical
perties, all distinguish it from Helvin and prove the mineral
be a new species. As such I take great pleasure in giving to
the name of *Danalite*, after Prof. James D. Dana, of New
ven, a name so honorably associated with American miner-
gy.

Small fragments of Danalite are not unfrequently met with in
quarries at Rockport, and small grains of it, as I have
eady said, are quite generally disseminated through the
anite ledges which form the extremity of Cape Ann. But
ge masses of the mineral have not been obtained for some
ne, the portion of the rock in which they were found having
en long since quarried. The mineral was first supposed by
e local collectors to be Rhodonite, and under this name good
ecimens of Danalite may probably be found in some of the
llections of the country. Specimens of the mineral have also
en found associated with green feldspar at the railroad cut
ar Gloucester, Mass. The mineral at this locality is more
rnet-like in structure, and contains a considerable amount of
umina associated with the glucina. An analysis of a specimen
m this locality gave the following results:

Silica,	29·88
Protoxyd of iron,	28·13
Oxyd of zinc,	18·15
Oxyd of manganese,	5·71
Glucina and alumina,	14·72
Lime,	0·83
Magnesia,	traces
Sulphur,	4·82
	102·24
Oxygen equiv. to sulphur,	2·41
	99·83

At Gloucester the Danalite is associated with fluor spar, which I
ave never recognized on the specimens from Rockport, although
e granite, in which the mineral is imbedded, has at both local-
ies a similar character. Danalite is also associated at both
calities with two very remarkable varieties of lepidolite mica.
hese have also been analyzed and an account of the investiga-
on will be given in a future paper.

Separation of Sesquioxyd of Iron from Alumina, Glucina, and most of the rare earths.—The method of Mr. H. Sainte-Claire Deville,[1] referred to in the above paper, is one of the most accurate processes of analytical chemistry, and would probably have been more generally used had it not been supposed that it required a furnace heat and involved the usual accompaniments of furnace work. The simple modification we have introduced is to substitute a tube of platinum for the tube of porcelain, and a Bunsen's blast lamp for the furnace. We use in the process the condenser of a small platinum still; but a smaller tube about 6″ long and $\frac{4}{10}$″ in diameter would be better adapted to the purpose, and would serve many other useful ends in the laboratory. In addition to the tube, a small platinum nacelle would be required, as large as the tube will admit and about 1½″ in length. With such an apparatus the method of conducting the process is as follows: The tube having been mounted horizontally on any convenient stand, one end of it, which is closed by a doubly pierced india rubber cork, is connected on one side with a small hydrogen generator and on the other with a small flask for generating hydrochloric acid gas. To the other end of the tube is fastened by an india rubber connector a small glass adapter, so curved that the end may dip under water. The mixed bases, whose total weight is known, having been placed in the nacelle in a finely pulverized condition, and the nacelle having been introduced into the tube, the heat of a single Bunsen burner is applied, while a gentle current of hydrogen is caused to flow through the apparatus. In the course of half an hour all the oxyd of iron is reduced to the metallic state. The current of hydrogen is then replaced by a much more rapid current of hydrochloric acid gas and the heat of a blast lamp applied. The reduced iron is now rapidly converted into chlorid, which, being volatilized by the heat and carried forward by the current, dissolves in the water. After a few minutes the action ceases, the heat is then withdrawn, and the current of hydrochloric acid gas being again replaced by a current of hydrogen, the apparatus is allowed to cool. The alumina, or whatever earth may be present, is left behind in the nacelle in a perfectly pure condition and can be at once weighed, while the weight of sesquioxyd of iron is known from the loss. If the product is not perfectly white the nacelle should be returned to the tube and the process repeated. The result can be controlled by also weighing the nacelle after the reduction of the iron, but it is not safe to estimate the amount of iron solely from the loss of weight at this time, since a very small error in this determination would cause an important error in the calculated amount of sesquioxyd. We give these details not as new, but because we feel assured that with the simple modification here described the process will be found far more expeditious, convenient and satisfactory, than any other process now in use. A small porcelain tube might be used instead of the tube platinum, but this cannot be recommended, as the porcelain is liable to break unless protected, and when properly protected sufficient heat can hardly be obtained without a furnace. The hydrogen gas is best obtained from a small automatic generator, and the hydrochloric acid gas may be generated in a small flask from coarse salt and sulphuric acid, which has been previously diluted with about one-third of its volume of water, and allowed to cool. This mixture when gently heated gives a constant flow of gas, which almost immediately stops when the lamp is withdrawn. Both gases should pass through a wash bottle containing strong sulphuric acid before entering the tube.

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[1] Annales de Chimie et de Physique, Tome xxxviii.

Art. XII.—*Memorandum of a variable or temporary Star of the Second Magnitude, seen in the Northern Crown, May*, 1866; by E. J. Farquhar, Assistant Librarian U. S. Patent Office.

Walking out between eight and nine o'clock in the evening of Saturday, May 12, near Sandy Spring, Montgomery county, Maryland, and looking over the constellations in the east, I was surprised at the appearance—or apparition I may call it—of a star in the Northern Crown which I could not believe I had ever seen there before. Immediately on reaching home I looked up an atlas of the heavens, and found no such star marked upon it. I then walked over to the house of my uncle, Mr. Benjamin Hallowell, who having looked at another map of his own, and found no record of such a star, came out with me to see it. As soon as I had pointed it out to him, he remarked that he had seen it for several nights, amounting to three weeks, or as he afterwards said, a month, probably ever since the constellation had come within view of a spot where he was accustomed to take an evening walk. He is therefore, so far as I know, the first person who ever saw it. He had remarked it as an unfamiliar star, and supposed it was a planet, without considering whether any planet ever frequented there. He did not think it had changed position at all during the month, but he was inclined to believe it had varied in magnitude from time to time; though on neither of these matters will he speak positively, because he had not given the star any special attention. It appeared to be two-thirds or three-quarters of a degree south of Epsilon Coronæ. It was of a pure, soft white, and twinkled a little. Seen through a telescope that magnifies about forty times, it showed nothing of the nature of a comet. I thought it grew brighter during that evening, but will not be certain. I believe those who carefully observed its magnitude pronounced it a little brighter than Alphacca, Alpha Coronæ. One observer seemed inclined to question this, but did not profess to be sure of his judgment in that respect. There can be no doubt that during at least part of that night, the stranger star was fully as bright as Alphacca; I think brighter. Sunday morning it did not seem to have changed in luster, but Sunday night it was only of the third magnitude, and since that time it has gradually faded from sight of the naked eye. On Tuesday night it was taken note of at the Washington Observatory, and I suppose it is therefore not necessary for me to carry this memorandum any further.

ART. XIII.—*New and Brilliant Variable Star;* by B. A. GOULD. (In a letter to the Editors dated Cambridge, June 9, 1866.)

ON Monday evening, May 14, Mr. S. C. Chandler, Jr., of the U. S. Coast Survey, while engaged in observing the magnitudes of fixed stars, by comparison without optical aid, perceived a brilliant star not a degree from ε Coronæ. At 11 P. M. he estimated its light as between that of β and γ Herculis, rather nearer to the latter; it was decidedly brighter than δ Boötis, and at least two-thirds of a magnitude brighter than β or γ Coronæ.

The sky had been obscured for several successive nights, but Mr. Chandler is confident that, three weeks previous (at which date he had examined the region with care), no star of sufficient brilliancy to attract attention was visible in this place.

On the ensuing evening, May 15, at 9 P. M., Mr. Chandler and myself examined the star together, and agreed in regarding its brilliancy as not essentially different from that of β Coronæ or γ Herculis, and as intermediate between the two. It was very manifestly fainter than δ Boötis.

The weather precluded further observations until May 19, on which evening the star had decreased by considerably more than two magnitudes, and was very near the limit of visibility to the naked eye. It was compared with several neighboring stars of similar brightness in Hercules and Serpens, both at 9^h and at 13^h; and in the interval between these comparisons it had diminished by not less than a tenth of a magnitude.

On the 20th, it was no longer perceptible by the unaided eye, but was easily seen and compared by means of an opera glass.

Subsequent observations have been made by Mr. Chandler and myself on the 24th, 28th, 31st May, and this evening, June 9; these being the only nights when the exceedingly unfavorible weather has permitted. Its magnitude this evening seems o be almost exactly the ninth.

The position of the variable was at once seen to correspond very nearly with that of a star, No. 2765 of 26°, given by Argelander in his "Durchmusterung des südlichen Himmels." An observation of position, by means of a transit-instrument belonging to the Coast Survey, and temporarily in my possession, corroborated the impression that these stars were identical; and now that the variable has waned to the 9th magnitude, and no other small star is found to have been obscured by its excess of brilliancy, it is manifest that the original suspicion was correct. There seems to be no regular observation of the star's place on record.

The determinations of magnitude during the time of visibility to the naked eye are rendered easy by means of a yet unpublished uranometry of the region between the declinations +45° and −2°, prepared at the Dudley Observatory in Albany during the year 1858, in which the brightness of every star visible to the naked eye is given to the nearest tenth of a magnitude. This, however, affords the numerical values for no date subsequent to May 19; and the comparison-stars for later observations are still subject to some uncertainty, which may affect the determination for the variable by a tenth or possibly by two-tenths of a magnitude. These will, however, be carefully determined before long by Mr. Chandler.

The Albany values for the brightness of the comparison-stars are these:

	M.		M.		M.
α Coronæ,	2·0	γ Herculis,	3·5	π Serpentis,	4·6
β Herculis,	2·3	β Coronæ,	3·5	B. A. C. 5399,	5·9
δ Boötis,	3·1	γ Coronæ,	3·6	Bessel Z. 296,3,	6·0
ε Herculis,	3·4	ε Coronæ,	3·9	B. A. C. 5452,	6·1

For the variable, the magnitudes, as thus far determined by us, are

		M.			M.
1866, May 14,	11h	2·9	1866, May 24,	9½h	7·8
15,	9	3·5	28,	10	8·9
19,	9	5·8	31,	10	8·9
"	13	5·9	June 9,	10	9·0
20,	9½	6·3			

Mr. Chas. A. Schott in Washington observed the star May 24 and 31, and estimated the magnitudes on those dates as 8·1 and 8·7 respectively.

Since first calling public attention to the sudden appearance of this remarkable star, I have received from many quarters information of its independent and, in several instances, previous detection; but only in a few cases do trustworthy determinations of its magnitude appear to have been made.

Mr. Wm. M. Davis, Jr., of Philadelphia, saw the star on the evening of May 12, called the attention of his family and friends to the phenomenon, and noted in his journal that the star was as bright as α Coronæ.

Mr. Ferguson, of the Washington Observatory, writes that the star was seen on Sunday evening, May 13, by Mr. Farquhar of Washington, assistant to Prof. Schaeffer, who communicated the fact to Admiral Davis, superintendent of the observatory. Mr. Farquhar estimated the magnitude on the 13th inst. as the second; Mr. Ferguson observed the star on the 15th, and estimated it as then of the fourth magnitude.

Prof. Watson, of the Ann Arbor Observatory, sends me word that Mr. Barker, a gentleman in London, Canada, perceived the star about May 1, and described it as equal to ε Coronæ in brilliancy at that time.

Prof. Henry Tutwiler, of Greene Springs, Ala., also detected the star on the 12th of May. For letters from him I am indebted to Robert Patterson, Esq., of Philadelphia, and to Prof. Henry of the Smithsonian Institution. He states that on that evening, it was somewhat superior in brilliancy to α Coronæ; and on other dates he observed or estimated it as follows: May 14th, 3d mag., somewhat brighter than β Coronæ; May 17th, less bright than ε Coronæ; May 19th, barely visible to the naked eye; May 20th, only perceptible through a small spy-glass, 8th mag.; May 24th, 10th mag. This last estimate must have been an extreme one, very possibly in hazy sky and without comparison-stars.

At an early day the star was also noted by Mr. Hallowell of Alexandria, who has very recently communicated his observations to a Philadelphia daily paper, but I have not yet been able to see them. Indirectly I have been informed that Mr. Hallowell has seen the star on previous occasions during the winter, which would imply that it has been fluctuating in short periods, since Mr. Chandler is positive that when he examined the region toward the close of April, the star was, to say the least, not conspicuous.

Mr. R. L. Knight, of Philadelphia, writes me that on the 23d of September last he saw, in the constellation of the Crown, a brilliant star, not laid down upon the maps, and that it was then equal to Gemma in brilliancy.

From these various data it would seem probable that the new variable which should, following Argelander's notation, receive the name *T Coronæ*, must have reached a magnitude of at least 1¾ at maximum, and that this maximum, perhaps only one of a series, occurred between the 5th and 12th of May.

P. S. June 12. The Astronomische Nachrichten of May 26, this day received, brings information of the detection of this star in Ireland on the 12th, and in Rochefort, France, on the 13th of May.

On the 16th, Mr. Huggins and Prof. Miller made a careful observation of its spectrum,—the star being then a little below the 4th magnitude. Their inference was that the spectrum was double, consisting of one principal system of lines analogous to that of the sun; and, superposed upon this, a second one, apparently due to light emanating from intensely heated gaseous matter,—containing, among other bands, two bright ones in the positions of the lines F and C, which correspond to hydrogen lines.

Mr. Courbebaisse, who observed the star at Rochefort on the 13th, states that he had seen no such star there on the 11th.

Art. XIV.—*On the Emery Mine of Chester, Hampden County, Mass., with remarks on the nature of Emery, and its associate minerals;* by J. Lawrence Smith, Pres't Louisville Gas Co.

Considerable interest is attached to the recent developments of an extensive deposit of emery in Chester, Hampden county, Mass., by Prof. C. T. Jackson; and my name has been associated in various ways with it, without my having had any thing directly to do with it. Sundry communications have also been received by me from various parties. These communications are best answered by the facts embraced in this article, some portions of which it has always been my intention to publish without reference to the special interest of any one in the matter.

Prior to 1846, emery was simply known as a mineral, coming to us from a few remote localities, and was used in the arts without our having any knowledge of its true geological position or its mineralogical relations. About that period, circumstances favored my commencing those geological and mineralogical discoveries in relation to emery, that were afterwards embodied in two papers, presented to the Academy of Sciences of Paris, in 1850, in which the subject was thoroughly discussed, and I might say almost exhausted. The light in which those discoveries were considered will be seen by the conclusions of the report of the committee of the Academy, consisting of Messrs. Dufrenoy, Elie de Beaumont, and Cordier, viz:

"It results from the review just given of the labors of Dr. Smith, that he has made known—

1st. The precise nature of the geology of emery in Asia Minor and the Grecian Archipelago;"

2d. "That he has described the properties of the principal minerals associated with it, and the manner in which they occur, especially diaspore and emerylite; this last mineral forms, by the identity of its composition in the different formations that the author had occasion to study, a mica constituting a new species, and one well determined;"

3d. "Finally, that he has given a means for determining the qualities of emery, and consequently their commercial value; this process, eminently practical, offers, besides, an interest in a scientific point of view, inasmuch as it permits of determining the difference in the tenacity of minerals of equal hardness.

"These researches of geology, mineralogy, and of analytical chemistry, constitute a work of the highest interest, both as a whole, as well as from the new facts they promise to science. Your committee consequently propose to thank Dr. Smith for having communicated them to the Academy, and in consideration of the importance of the work, to order the insertion of his paper in the *Receuil des Mémoires des Savants étrangers.*"

At that time I had discovered six new localities of emery in Asia Minor, and the Grecian Archipelago. Those localities were far removed from each other, and furnished so many different places for the study of emery and its associate minerals in addition to the old locality of Naxos; and consequently many points of general interest were brought out, besides others connected with the line of study. Those who may feel interested in the subject will find the investigation and results there arrived at in this Journal, vols. x and xi, 1850 and 1851; they embrace the geology, mineralogy, chemical composition, manner of mining, commercial considerations, associate minerals, &c.

The study of the associate minerals I considered of great importance, as they would be guides in future explorations in other parts of the world; and even prior to completing the researches on the subject I wrote to Professor Silliman and asked him to examine the American corundum localities for these minerals, one of them in particular, which he immediately did. With the corundum from the locality in Chester county, Penn., and Buncombe county, N. C., he "soon found the mineral indicated," and communicated the same to this Journal, Nov. 1849, pp. 379 and 383.

Nothing further came to my notice in relation to emery until I received from Prof. C. T. Jackson a letter dated Oct. 9th, 1864, containing what follows:

"You discovered emerylite or margarite in Asia Minor as an associate mineral with emery. On the 22d of October last, 1863, I discovered, while surveying an iron mine in Chester, Mass., some beautiful veins of the margarite, from half inch to two inches wide, and of a fine delicate rose color, or light pink. The nature of this mineral I did not discover until my return to Boston, but at first supposed it was lepidolite; on analysis it proved o be margarite, and from that I ventured to predict the occurence of emery, but no attention was paid to this prediction by he owners of the mine, who were more intent on the iron ore. A few weeks since, I saw Dr. Lucas, one of the owners, resident in Chester, and called him into my office, and explained to him the great value of emery, and told him how to detect it, and he promised to make the search I required, and took exact directions from me."

"The next day after his return to Chester, he found the emery, a big vein nearly six feet wide, which had been mistaken by him for iron ore, it being very magnetic. I write you this, to show you the importance of your discovery of the emerylite or margarite (for this appears to be identical), as an associate of emery, and also as an interesting case of deduction from scientific memoirs."

Accompanying the letter he sent me a paper giving me a summary of a communication he had made to the Boston Society of Natural History on the subject, concluding by remarking that "had not the occurrence of emerylite and chloritoid called his attention to the probable existence of emery at this locality, it would have been overlooked to this day, and no one knows how much longer. The fact was mentioned as an example of the real uses of supposed useless minerals; and the Doctor took occasion to express his obligations to Dr. Smith, of Louisville, for his valuable contributions to our knowledge of the associate emery minerals of the Grecian Archipelago and Asia Minor."

These statements are sufficient to show how far my geological observations served as a guide to Prof. C. T. Jackson, in his deductions with reference to the existence of emery in Chester, and with what diligence Dr. H. S. Lucas followed up the latter's directions, resulting in the valuable development of emery.

I have since visited the locality, having done so in the month of March last. The geological character and position of the rocks was not as well made out by me as might have been done in a more favorable season; but as my observations accord, as far as they go, with those of Dr. Jackson and Prof. Shepard, I prefer inserting their observations, rather than my own, in describing the geology of the emery locality.

"The mine is situated nearly in the center of the Green Mountain chain as it traverses the western border of the state, at a point not far from half way between the Connecticut and Hudson rivers. It is included in the metamorphic series of rocks, here consisting of vast breadths of gneiss and mica-slate, with considerable interpolations of talcose slate and serpentine. The general direction of the stratification is N. 20° E. and S. 20° W., the relation to the horizon varying from vertical, to a dip of from 75° to 80°, sometimes east, sometimes west.

"The immediate vicinity of the mine presents a succession of lengthened rocky swells with rather precipitous sides, having summits between 750 and 1000 feet above the level of the principal streams by which the hills are traversed. The longer axis of the elevations generally coincides with the directions of the strata.

"The emery vein traverses in an unbroken line the crests of two of these adjoining mountains, and scarcely deviates as a whole from the magnetic meridian. Each mountain is estimated to have a length of two miles, thus giving four miles extent to the metalliferous stratum, for such it may be truly called, consisting as it does so largely of the metals iron and aluminum. The Westfield river, here a small stream about four rods in width, flows directly across the northern end of the vein, dividing it into two equal portions. The height of each mountain is estimated at 750 feet.

"The emery vein, whose average width may be taken at four feet, is situated near the junction of the great gneiss formation constituting the western flank of the mountains with the mica-slate forming the eastern slope. To speak more exactly, however, it lies just within the gneiss, having throughout a layer of this rock of from four to ten feet in thickness for its eastern wall. Nor does the mica slate advance quite up to this outside layer of the gneiss; but in place thereof, an extensive intrusion of talcose slate occurs, having an average thickness of twenty feet on the south mountain, and widening out at the north mountain to a breadth of nearly 200 feet as it reaches the terminus of the vein, in the bed of the Westfield river.

"The gneiss, more especially in the vicinity of the vein, is a very peculiar rock. It abounds in thick seams of a coarse-grained, very black and shining hornblende; and where this is not found, it is much veined and penetrated by epidote. The stratification is much contorted also; and when the surface of the formation happens to be weathered or water-worn, its basseting edges strikingly resemble in color some of the serpentine marbles. It is also noticeable that in it quartz is everywhere singularly deficient. Traces of a white calcareous spar (calcite) are now and then visible upon the joints of the gneiss, with occasional specks of yellow copper, together with malachite stains; but no corundum, emery, or magnetite particles have thus far been detected as constituents of the gneiss. It is quite otherwise, however, with the talcy rock exterior to the wall of gneiss; for that formation in all its different varieties of talcose slate, soapstone, chloritic aggregates (with included seam of indianite), talcy dolomite, &c., which together constitute the stratum separating the gneiss from the mica slate, contain here and there disseminated grains of either emery, corundum, or magnetite; but, ke the gneiss again, are strikingly free from quartz or uncombined silica in any of its forms. Indeed this generally abundant ubstance is altogether wanting, not only in the emery-vein but n the talcose formations constituting its eastern boundary.

"It makes its appearance, however, in abundance in the mica late as soon as the talcose rocks are passed—showing itself not nly as the usual constituent of the slate, but in more or less ontinuous seams, from a few inches thick up to above six inches, nd sometimes a foot, in width. Where the seams are thin and liscontinuous, the included masses thin out at each end before lisappearing, the sharp edges being curved in opposite directions, so as to form frequent white patches upon the surface of the rocks in the shape of the letter S."

Mineralogical Character and Composition of the Chester Emery.

It resembles more nearly that from Gumuchdagh (near Ephesus,) than any other that I know of. It is of a fine grain, and dark blue bordering on black, not unlike certain varieties of magnetic iron ore; with it there are frequently found pieces of corundum of some size. The interior of the mass is free from micaceous specks, such as are found in the emery of Naxos. Its powder examined under the microscope shows the distinct existence of more than one mineral, which are often so inseparably connected that the smallest fragments contain them together. The two predominating are *corundum* and *magnetic oxyd of iron.*

Several specimens were submitted to chemical examination from those most largely impregnated with magnetic oxyd of iron to those that appeared to contain least. They all consisted essentially of alumina and oxyd of iron; but I invariably found a little titanic acid and silica, and most commonly a minute quantity of magnesia. No. 1 was an inferior specimen; No. 2, the better quality of rock; No. 3, the emery rock crushed and prepared for market in the form of emery; No. 4, the same, and called emery crystals.

	1.	2.	3.	4.
Alumina,	44·01	50·02	51·92	74·22
Magnetic oxyd of iron,	50·21	44·11	42·25	19·31
Silica,	3·13	3·25	5·46	5·48

I examined a specimen of No. 2: grain fine, and treated repeatedly with hydrochloric acid and water over a water-bath: a great deal of oxyd of iron and a little alumina were dissolved; the residue on analysis proved to be nearly pure corundum, giving,

Alumina, - - - - - - -	84·02
Magnetic oxyd of iron, - - - - -	9·63
Silica,[1] - - - - - - - - -	4·81

[1] No attempt was made to estimate the water.

All the chemical and physical examinations made go to show that the emery of Chester is, like all other emeries, a mixture of corundum and oxyd of iron; a fact that will be reverted to again a little farther on.

Prof. Jackson analyzed two specimens, after digesting them with nitro-muriatic acid, and has given as the composition,

	1.	2.
Alumina, - - - - -	60·40	39·05
Protoxyd of iron, - - - -	39·60	40·95

and then goes on to state, "from which it would appear that protoxyd of iron is an essential chemical ingredient in emery, and not an accidental admixture.[1] Dr. J. Lawrence Smith's experiments lead to the same result, but he considers the oxyd of iron to be an irregular mixture with the alumina and not a regular chemical constituent. In either case, I think emery ought to rank as a separate species, and not as a granular variety of corundum, from which it differs so in physical characters."

I would here remark that Dr. Jackson's conclusion would be correct in the first state of the case, were the iron an essential chemical ingredient; but in the latter, it would be erroneous, and introduce inextricable confusion into the science of mineralogy by admitting mere mechanical mixture as a specific distinction.

Prof. C. U. Shepard writing on the same point says, "His conclusions (Dr. Jackson's) would obviously be acquiesced in were it not for the strong resemblance in striæ and cleavage between the emery and common corundum, making it impossible for us to separate the substances crystallographically from one another. Nothing like a perfect crystal of emery has yet been found at the mine; but it is quite remarkable that the mineral is here generally coarsely massive, or in large separate individuals often of the size of kernels of Indian corn, whose cleavages are perfect, and which present on their planes the delicate striæ so characteristic of corundum from the Carnatic." Yet Prof. Shepard is for making emery a new mineral species and calling it *Emerite*, with the formula $\dot{\mathrm{Fe}}\overline{\dddot{\mathrm{Al}}}$.

If the views of Profs. Jackson and Shepard are to be taken as correct, the question as to the mineralogical position of emery is easily settled without resorting to any new mineral species. It is simply a massive iron-spinel (hercynite) with the anomaly of having a hardness equal to corundum.

		Emeries,	
	Iron spinel.	Jackson.	Shepard.
Alumina, - -	58·75	60·49	$\overline{\dddot{\mathrm{Al}}}\dot{\mathrm{Fe}}$
Protoxyd iron, -	41·25	39·60	

I would say, at this point, that if the mineral of Chester is to be regarded as an aluminate of iron, the rock called emery coming from Naxos and other well known localities is not that compound, *and that if one is emery the other is not.* But as I do not take their view of the matter, *I consider the Chester mineral as true an emery as that of Naxos.*

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[1] An examination of my analyses in 1850, which it is supposed are the ones referred to here, most certainly do not sustain the conclusion.—J. L. S.

As there seems to be some mistake and incorrect quotation in regard to my analyses of emery and corundum, I reproduce the tabular statement of the analyses, and effective hardness, referring the reader to the original paper for a correct view of what is understood by the effective hardness.

No.	Locality.	Effective hardness Sapphire 100.	Specific gravity.	Composition.				
				Water.	Alumina.	Magnetic oxyd of iron.	Lime.	Silica.
	EMERY.							
1	Kulah, ...	57	4·28	1·90	63·50	32·25	0·92	1·61
2	Samos, ...	56	3·98	2·10	70·10	22·21	0·62	4·00
3	Nicaria, ...	56	3·75	2·53	71·06	20·32	1·40	4·12
4	Kulah, ...	53	4·02	2·36	63·00	30·12	0·50	2·36
5	Naxos, ...	46	3·75	4·73	58·53	24·10	0·86	3·10
6	Nicaria, ...	46	3·74	3·10	75·12	12·06	0·72	6·88
7	Naxos, ...	44	3·87	5·47	69·46	19·08	2·81	2·41
8	Ephesus, ...	42	4·31	5·62	60·10	33·20	0·48	1·80
9	Kulah, ...	40	3·89	2·00	61·05	27·15	1·30	9·63
	CORUNDUM.							
1	Sapphire of India,	100	4·06	....	97·51	1·89	....	0·80
2	Ruby " "	90	....	....	97·32	1·09	....	1·21
3	Corundum, Nicaria,	77	3·88	1·60	92·39	1·67	1·12	2·05
4	" Asia M.,	65	3·92	0·68	87·52	7·50	0·82	2·01
5	" Asia,	60	3·60	1·66	86·62	8·21	0·70	3·85
6	" India,	58	3·89	2·86	93·12	0·91	1·02	0·96
7	" Asia,	57	3·80	3·74	87·32	3·12	1·00	2·61
8	" India,	55	3·91	3·10	84·56	7·06	1·20	4·00
	EMERY.							
1	Chester, Mass., ...	33	....	....	44·01	50·21	....	3·13
2	" "	40	....	....	50·02	44·11	....	3·25
3	" "	39	....	....	51·92	42·25	....	5·46
4	" "	45	....	....	74·22	19·31	....	5·48
5	" "	..	....	....	84·02	9·63	....	4·81

By the above it will be seen that the magnetic oxyd of iron in the emery of Naxos, Ephesus, &c., varies from 13 to 33 per cent, the water from 1·9 to 5·0 per cent, the silica from 1·6 to 9·6 per cent. All of these ingredients form minerals apart from the corundum, which is represented by the principal portion of alumina. Some of the alumina found in the analysis is associated with the above ingredients to form associate minerals which have been fully studied. This last will serve to explain why it is that emeries having the same amount of alumina may have different degrees of effective hardness. Thus: Nos. 9 and 4, both Kula emeries, containing about the same amount of alumina, have effective hardness in the proportion of 40 to 53; but it will be seen that No. 9 contains 9·6 per cent of silica, which doubtless appropriates a portion of the alumina, thus reducing the alumina attributable to corundum; so that, were it possible to ascertain the exact amount of corundum present in 9 and 4, it would doubtless be in proportion to their effective hardness. So again, if we compare Nos. 8 and 1, the effective hardness will be found in the proportion of 42 to 57, while their amounts of alumina vary only as 60 to 63; but if we regard the amount of water in the two it is as 5·6 to 1·9, much of this water coming from diaspore that is intimately mixed with the corundum; and in several specimens I possess, the two minerals shade into each other so completely, that it is impossible to tell where one begins and the other ends. The above facts were all well examined when my first memoirs appeared on this subject, which accounts for the following remark then made:

"Those emeries which contain the least water, everything else alike, are the hardest, as instanced by that from Kulah, notwithstanding the quantity of iron it contains. The silica existing in emery is most often in combination with alumina, or the oxyd of iron, or both; for this reason we must not always regard the quantity of alumina as an indication of the quantity of corundum in emery."

In concluding this part of the subject I would state that while I do not consider my opinions infallible in this matter, still all my experience and research, gathered from such varied sources, point to the conclusion that emery is a mixture of several minerals, principally corundum and magnetic oxyd of iron, the former being the effective agent in the mechanical abrasion to which it is applied; the oxyd of iron is not to be considered as an unimportant ingredient, it serving by its presence to destroy to some extent the harsh cutting action of the corundum.

Minerals associated with the emery of Chester.

Corundum.—This mineral, as might naturally be expected, is found with the emery, sufficiently distinct and separate to be at once recognized, sometimes in thin seams, massive in its character, but more commonly in flattened crystals of small dimensions.

Diaspore.—Very excellent and beautiful specimens of this hydrate of alumina have been found at this emery locality; it is often in distinct and separate prismatic or bladed crystals, quite colorless and transparent.

Emerylite or Margarite.—Some of the finest specimens of this mineral that are known have been found at this locality. It will be seen by referring to my former papers on emery, that I first discovered this mineral associated with emery; its composition showed it to differ from any other then known mineral. I compared it subsequently with margarite, which had been discovered before, and suspected the identity of the two minerals;

but as the analysis made out and accepted as the composition of margarite did not accord with that of emerylite, I undertook to reëxamine margarite, when I found that its composition had been erroneously determined, and that it was, in fact, the same mineral with emerylite, which last name has had to yield to the priority of date of the other.

I have analyzed the margarite from Chester and find its composition as follows:

Silica,	32·21
Alumina,	48·87
Lime,	10·02
Oxyd of iron,	2·50
Manganese,	·20
Magnesia,	·32
Soda and little potash,	1·91
Lithia,	·32
Water,	4·61

There is a little titanic acid with the oxyd of iron that I did not estimate.

Chlorite.—This mineral as found with the emery is the so-called *corundophilite* of Shepard; on examination it proves to be, both chemically and physically, a chlorite of the variety ripidolite.

Biotite.—In examining a specimen of dark green micaceous mineral which I took to be chlorite (the corundophilite of Shepard) and from its purity expected to get a very accurate idea of its composition, but in the very commencement of the examination it was discovered to be well characterized *biotite.*

This mineral occurs on the surface of a white rock that Prof. Shepard calls indianite, but which I have not had time to examine. It is in small thin micaceous crystals perpendicular to the surface of the indianite; in the mass, it is of a dark green color, so dark that at a little distance it looks like lamellar plumbago. A careful analysis gave the following composition:

Silica,	39·08
Alumina,	15·38
Magnesia,	23·58
Peroxyd of iron,	7·12
Manganese,	·31
Potash,	7·50
Soda,	2·63
Water,	2·24
Fluorine,	·76
	98·60

This corresponds with the composition of the biotite from Monroe county, New York, as made out by Prof. Brush and myself in our reëxamination of American minerals several years ago, *Corundophilite proved to be a chlorite.*—About the time I published my memoirs on emery in 1850 and 1851, Prof. Shepard made the announcement of a new mineral (this Journal, 1851, xii, 211), stating that it "occurs with corundum near Asheville, in Buncombe Co., N. C., in imperfect stellate groups, and also spreading out in laminæ between layers of corundum; color leek-green, etc." An analysis of it afforded silica 34·76, protox. iron 31·25, alumina 8·55, water 5·47, *making a loss of nearly* 20 *per cent, a portion of which he attributes to alkalies;* neither lime nor magnesia were detected. He operated on 140 milligrams; this mineral was considered a new one, and Prof. Shepard called it corundophilite. Supposing that I had observed the same mineral in certain specimens of emery and emerylite from Chester, Mass., I enclosed a fragment of the specimen to Prof. S. to ascertain if this was the mineral he called *corundophilite;* he returned the specimen, announcing that it was. I then analyzed the same and found it to be, both chemically and physically, a chlorite, identical no doubt with the chlorite I found associated with the emery of Asia Minor; both the Asia Minor and Chester varieties occur in compact mass, composed of an agglomeration of small crystalline plates—identical with the chlorites of Mont des Sept Lacs and of St. Christophe, and the ripidolites of Rauris and St. Gothard. In the following analysis I do not pretend to furnish that of the pure mineral, as from the thinness of the layers in the specimens at my disposal it cannot be separated in that state of purity I am in the habit of seeking for in all minerals that I examine:

Silica,	25·06
Alumina,	30·70
Protoxyd of iron,	16·50
Magnesia,	16·41
Water,	10·62
	99·29

The optical characters were not examined, there being no means at hand.

I may remark that the alumina and magnesia were separated by resolution and reprecipitation three times.

Tourmaline.—This mineral is also found with the emery of Chester in the same manner as with the emery of Naxos.

Titaniferous iron (ilmenite).—This is found principally in flattened crystals in the margarite.

Oxyd of titanium (brookite or rutile).—With the diaspore we found some beautiful flattened hair-brown crystals; the specimen in my possession does not furnish the face of the crystals so as to enable me to make out what form of titanium oxyd it is. Prof. Shepard thinks he has sufficient evidence to pronounce it to be brookite.

Magnetic oxyd of iron.—This ore of iron is found in great abundance associated with the emery, and is worked for the manufacture of iron; it contains a little oxyd of titanium.

The above, as well as some other associated minerals of less importance, justify the concluding remarks of my paper on emery fifteen years ago, viz: "I do not risk much in saying that the hydrate of alumina or diaspore, as well as the silicate or emerylite, chlorite or tourmaline, and the minerals of iron, as magnetic, titaniferous iron, &c., will be found almost everywhere with the emery and corundum."

ART. XV.—*On some minerals associated with the Cryolite in Greenland;* by G. HAGEMANN.

A NOTICE of the pachnolite, discovered by Prof. Knop in the Greenland cryolite, has already appeared in this Journal.[1] On examination of several cargoes of cryolite imported by the Pennsylvania Salt Manufacturing Company, I have not only found pachnolite, but also have observed some other minerals which may be of interest.

Dimetric Pachnolite.—Among these is a mineral first observed by Prof. Julius Thomsen of Copenhagen, the originator of the cryolite industry. As I am informed, he found a mineral, which on a preliminary examination he thought might prove a fluorid of silicon compound, but I have not heard of any further investigation of the substance. In looking over the cargoes of cryolite I have found a mineral which I believe to be the same with that noticed by Prof. Thomsen.

The mineral crystallizes in dimetric form, the dimetric pyramid and prisms being plainly seen, but no further crystallographic examination was made. It has a distinct basal cleavage. The color is white with a reddish tinge, the crystals have a bright luster, and are coated with a white earthy envelop ($\tilde{\mathrm{S}}\mathrm{i}$?). Sp. gr., 2·74–2·76; hardness, about that of cryolite. Heated in the closed tube this mineral yields water with an acid reaction which etches the glass. At a higher temperature it melts to a clear glass, fusing even more readily than cryolite. When pulverized it is easily decomposed by sulphuric acid, and on qualitative analysis it proves to contain water and fluorine, aluminum, calcium, sodium, and some silica. In the quantitative examination it was found extremely difficult to separate the alumina from the lime when precipitated by ammonia from the solution in sulphuric acid. I redissolved and reprecipitated six times before obtaining a complete separation. The water was determined by heating the mineral with previously ignited quicklime. The fluorine was determined as fluorid of calcium by decomposing the mineral with a mixture of silica, and the carbonates of potash and soda. After the soluble fluorids were separated from the insoluble silicates, alumina and silica were separated by carbonate of ammonia, and fluorid of calcium with carbonate of lime were thrown down with chlorid of calcium; this precipitate was dried and ignited, and the carbonate of lime was removed by acetic acid. The silica was imperfectly determined, as I had not the means at my disposal to estimate it accurately. I treated the pulverized mineral with solution of soda and carbonate of soda, filtered, and decomposed the solution by chlorhydric acid; evaporating to dryness thus rendering the silica insoluble. Analysis gave,

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[1] Vol. xli, p. 119.

		Equivalents.	
Fluorine,	50·08	2·63	
Aluminum,	14·27	1·05	
Sodium,	7·15	0·311	1·036
Calcium,	14·51	0·725	
Water,	9·70	1·07	
Silica,	2·	0·135	
	97·71		

The formula is very near $Al_2Fl_3+2(\frac{2}{3}Ca+\frac{1}{3}Na)Fl+2HO$, which corresponds closely with Knop's formula for pachnolite, $Al_2Fl_3+3(\frac{3}{5}Ca+\frac{2}{5}Na)Fl+2HO$. I scarcely know how to place the silica, but I think it does not really belong to the compound.

Arksutite.—This is a white crystalline granular mineral with a high luster. No crystals were observed, but each grain shows at least one good cleavage. Sp. gr., 3·029–3·175, (variation probably caused by minute crystals of iron-pyrites). Hardness, the same as cryolite. Fuses at a red heat without giving off water. Analysis gave,

Fluorine,	51·03	2·68		2
Aluminum,	17·87	1·307		1
Sodium,	23·00	1·	1·35	1
Calcium,	7·01	0·35		
Moisture,	0·57			
Insoluble,	0·74			
	100·22			

Hence the compound gives the formula $Al^2Fl^3+2(Ca,Na)Fl$. I hope in time to find a series of these fluorids which will, perhaps, show how cryolite is decomposed into pachnolite, and this may be still further altered into what the Greenlanders call "natural soap," (a hydrate of alumina?). Both these minerals are found associated with cryolite in the vicinity of Iviktant near Arksut-fiord, in South Greenland.

Natrona, Pa., May, 1866.

ART. XVI.—*Evidence of Two distinct Geological Formations in the Burlington Limestone;* by W. H. NILES and CHARLES WACHSMUTH.[1]

DR. CHARLES A. WHITE was the first to record any natural division of the Burlington limestone. In the Journal of the Boston Society of Natural History, vol. vii, No. 2, Dr. White has given a "Section of rocks exposed at Burlington." He there describes eight beds, which he numbers from the lowest upward. He refers the first six beds of his section to the Chemung group, and beds "No. 7" and "No. 8" to the Burlington limestone. In vol. ix of the Proceedings Bost. Soc. Nat. Hist., and in No. 4 of the Journal of the same society, Dr. White describes certain species of fossils from the Burlington rocks; and although he gives the beds or divisions in which the species occur, yet nowhere does he claim that the Burlington limestone comprises more than one geological formation.

Our own observations have led us to regard these two divisions of the Burlington limestone as two distinct geological formations. The lower division we call the Lower Burlington limestone, and the upper division, the Upper Burlington limestone. The reasons for ranking these divisions as distinct formations are as follow:

The Burlington limestones are eminently crinoidal in their composition, as well as in their better preserved fossils. While fragments of these remains form an important feature in the greater mass of these rocks, there are, likewise, some strata of considerable thickness, which are composed almost entirely of these fragments. We know about three hundred species of these fossils from the immediate vicinity of Burlington alone. These species represent twenty-four recognized genera, and were they classified according to their zoölogical characters the number of the genera would be considerably increased. Considering the age and the highly fossiliferous nature of these strata, the Mollusca, even the Brachiopoda, are represented by comparatively few fossil forms.

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[1] While making a special study of a family of Crinoids, the Actinocrinidæ, the results of which were to have published in the Illustrated Catalogue of the Museum of Comparative Zoology, it became necessary to spend considerable time at Burlington, Iowa, for the purpose of studying the large and valuable collections of Mr. Charles Wachsmuth, Rev. W. H. Barris, and Dr. Otto Thieme. From the published observations of Dr. C. A. White, and from some notes made by Mr. Wachsmuth, I had been led to believe that a careful study of the distribution of the crinoidal remains at Burlington would be rewarded with interesting paleontological results. I found Mr. Wachsmuth acquainted with many important facts in this connection, which could be reached only by a long experience in collecting these fossils, by an intimate acquaintance with the species, and a series of most careful observations. Accordingly, with the consent of Prof. Agassiz, I associated myself with Mr. Wachsmüth, for a careful examination and comparison of all the specimens in the three collections, for the identification of the species, and for the determination of their stratigraphical position; with the intention of publishing, in some scientific journal, such results as were of scientific interest.

We now give only a preliminary notice of some of the results of our study, designing to extend our investigations to more southern localities, where the Burlington limestone is exposed. In a future paper, we shall present a complete catalogue of all the described species of Crinoids occurring in these limestones, with notes upon their geographical distribution, their stratigraphical range, and the rarity or frequency of their occurrence. W. H. N.

The evidence that crinoids were the reigning forms of animal life in the waters of that ancient sea which deposited these limestone strata is of the most satisfactory nature. It is, therefore, to these animals that we ought to refer, for that evidence of organic change and progress characteristic of different periods in geological history. Such an organic change and progress is distinctly marked in the crinoidal remains of the Burlington limestones, and it is upon such evidence that we found our classification of these strata.

The strata of the Lower Burlington limestone present many differences in color, structure, and composition; but by intimate acquaintance they can generally be distinguished from those of the Upper Burlington limestone by their lithological characters alone. In the upper part of this formation the limestone strata become interstratified with beds of chert, and the uppermost stratum of chert, which attains any considerable extent and thickness, forms the division between the Lower and the Upper Burlington limestones. This stratum of chert, in the vicinity of Burlington, is from two to three feet in thickness. The Crinoids found below this stratum of division, are, in the aggregate, of smaller size than those above it; they are not so coarse in their general features, and their ornamenting ridges, nodes and spines, never assume that striking prominence exhibited in many species of the Upper Burlington limestone.

These features distinctly show that, during the deposition of these strata of the Lower Burlington limestone, the circumstances were less favorable to the extraordinary growth of these animals than they were during the time represented by the Upper Burlington strata. A similar, but not a more marked distinction of general features, is to be noticed between the crinoids of the Upper Burlington and those of the Keokuk limestone; as it appears from the fossils, that it was during the latter formation that crinoids culminated in extravagance of size and features. Three grades of crinoidal development are thus exhibited: by the species of the Lower Burlington, those of the Upper Burlington, and by those of the Keokuk limestone.

We have examined the species of Crinoids and noticed their stratigraphical distribution with care, and have found no evidence of any species occurring in both the Lower and the

Upper Burlington limestones. It would seem, from these facts, that there was something connected with the presence of siliceous matter in depositing waters, during the formation of the upper beds of the Lower Burlington Limestone, which was unfavorable to the growth and life of the inhabiting Crinoids; for, with the introduction of the chert deposits, the Crinoids appear to have declined, and finally all of the species became extinct before the completion of the dividing stratum of chert above mentioned. A parallel instance is to be noticed in the fact that there is a stratum of chert between the Upper Burlington and the Keokuk limestones, which marks a similar organic change and progress.

There was also a great change in the Mollusca, as most of the species of the two divisions are distinct.

We here give lists of some of the better known species of Crinoids, arranged under the names of the formations to which they are exclusively restricted. We are also acquainted with many undescribed species, which are as distinctly limited to one formation as those here mentioned.

Some species of Crinoids which are found only in the Lower Burlington Limestone.

Actinocrinus proboscidialis Hall.
multibrachiatus "
sexarmatus "
thetis "
ornatus "
inflatus "
sculptus "
discoideus "
turbinatus "
papillatus "
formosus "
inornatus "
lepidus "
aequalis "
opusculus "
chloris "
clarus "
infrequens "
lucina "
thoas "
ovatus "
cœlatus "
gemmiformis "
corbulis "
coronatus "
clio "
carica "
pentagonus "
unispina "
subaculeatus "
excerptus "
spinobrachiatus "

Actinocrinus divergens Hall.
superlatus "
brevis "
unicornis Owen & Shumard.
araneolus Meek & Worthen.
Agaricocrinus corrugatus Hall.
planoconvexus Hall.
Megistocrinus Evansii Owen & Shumard.
Whitei Hall.
Platycrinus planus Owen & Shumard.
Yandellii "
corrugatus. "
discoideus "
Burlingtonensis "
Prateni Worthen.
ornogranulus McChesney.
scobina Meek & Worthen.
verrucosus White.
regalis Hall.
eminulus "
parvinodus "
pileiformis "
subspinosus "
pocilliformis "
nucleiformis "
truncatulus "
exsertus "
calyculus "
cavus "
truncatus "
sculptus "
clytis "

Platycrinus excavatus Hall.
striobrachiatus “
nodobrachiatus “
Cyathocrinus Wachsmuthi Meek & Wor.
Iowensis Owen & Shumard.
cornutus “
rigidus White.
malvaceus Hall.
divaricatus “
solidus “
latus “
macropleurus “
Poteriocrinus æqualis “
calyculus “
subimpressus M. & W.
enormis “
Scaphiocrinus simplex Hall.
spinobrachiatus “

Scaphiocrinus ramulosus Hall.
tortuosus “
Wachsmuthi M. & W.
solidus “
Cœliocrinus dilatatus White.
Ichthyocrinus Burlingtonensis Hall.
Bursacrinus confirmatus White.
Forbesiocrinus Thiemei Hall.
Rhodocrinus Wortheni “
Wachsmuthi “
Whitei “ [Ha
“ var. Burlingtonens
Trematocrinus reticulatus Hall.
fiscellus “
Zeacrinus scoparius “
Pentremites stelliformis O. & S.
melo “
“ var. projectus M. & W

Some species of Crinoids which are found only in the Upper Burlingt Limestone.

Actinocrinus umbrosus Hall.
ægilops “
rudis “
insculptus “
tholus “
althea “
regalis “
glyptus “
perumbrosus “
ventricosus “
verrucosus “
rusticus “
glans “
calyculoides “
clœlia “
quinquelobus “
trinodus “
cornigerus “
decornis “
symmetricus “
subturbinatus M. & W.
asteriscus “
Konincki Shumard.
Verneuilianus “
Missouriensis “
multiradiatus “
subventricosus McChesney.
Wachsmuthi White.
Nashvillæ var. subtractus W.
Hageri McChesney.
Christyi Shumard.
pyriformis “
rotundus Yandell & Shum.
dodecadactylus M. & W.
oblatus Hall.
amplus M. & W.
Megistocrinus plenus White.
Agaricocrinus pentagonus Hall.
ornatrema “

Agaricocrinus excavatus Hall.
bullatus “
stellatus “
geometricus “
gracilis M. & W.
Platycrinus olla Hall.
glyptus “
tuberosus “
subspinulosus “
Wortheni “
quinquenodus White.
pleurovimenus “
asper Meek & Worthen.
incomptus White.
Dichocrinus plicatus Hall.
scitulus “
liratus “
lachrymosus “
striatus Owen & Shumard.
angustus White.
crassitestus “
Synbathocrinus dentatus O. & S.
Wortheni Hall.
papillatus “
Cœliocrinus subspinosus White.
Cyathocrinus viminalis Hall.
rotundatus “
sculptilis “
lamellosus White.
Poteriocrinus ob-uncus “
salignoideus “
Swallowi M. & W.
tenuibrachiatus “
carinatus “
Scaphiocrinus rusticellus White.
carinatus Hall.
Halli “
Rhodocrinus Barrisi “ [Hal
“ var. Burlingtonens

rematocrinus typus Hall.
tuberculosus "
papillatus "
entremites Norwoodi Owen & Shum'd.
elongatus Shumard.
Sayi "
sirius White.
orbesiocrinus Agassizi Hall.
asteriæformis "
ramulosus "

Cheirocrinus dactylus Hall.
ventricosus "
lamellosus "
Belemnocrinus typus White.
Bursacrinus Wachmuthi Meek & Wort'n.
Zeacrinus elegans Hall.
ramosus "
perangulatus White.
sacculus "
Troostanus Meek & Worthen.

ART. XVII.—*On a proposed Printing Chronograph;* by C. A. YOUNG, Prof. Nat. Phil. and Ast. in Dartmouth College.

EVERY astronomical observer who has used the ordinary forms of chronograph knows that at least as much labor is expended in measuring up the sheets, and putting the record into figures, as in making the observations; to say nothing of the fact that the operation involves an opportunity for accidental errors very vexatious and hard to trace.

It is proposed to save the whole of this purely clerical labor by making the chronograph itself record the instant of observation in hours, minutes, seconds and hundredths of a second, in printed characters, and in a form suitable for preservation and reduction.

This idea occurred some years ago to Mr. Hilgard of the Coast Survey, and at a recent meeting of the National Academy he read a description of an apparatus designed to realize it. Prof. Hough of the Dudley Observatory has also done something toward a solution of the same problem. The plan about to be proposed, however, differs from both of theirs, and was substantially devised nearly a year ago, previous to any knowledge that others were at work on the same subject.

Mr. Hilgard's invention undoubtedly possesses more of novelty and theoretical beauty; in practical accuracy there would be no difference, as either secures a record correct to the nearest hundredth of a second; in simplicity and cheapness of construction, certainty of operation, and security against derangement, the advantage is probably with the apparatus whose description follows.

The well known spring-governor of Professor Bond is taken as the basis of the instrument. The wheel-work should, however, be made much heavier than ordinary, and especially the centrifugal fly should be of sufficient size and weight to answer the purpose of a balance wheel in preventing sensible changes of velocity from slight momentary variations in the resistances of the train.

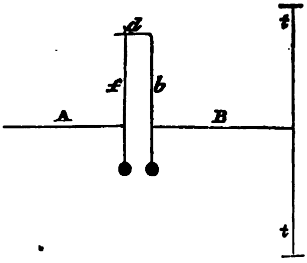
The pendulum should be controlled by electro-magnetic impulses sent every second from the standard clock, in the same manner practised for some years at the Dudley and other observatories. It might perhaps be well to substitute for the half seconds pendulum commonly used, a pendulum beating quarter seconds, the requisite rapidity of oscillation being secured by a spring.

I think that the experience of observers with the spring-governor chronograph warrants the belief that the irregularities of motion need never, in a well made instrument, cause errors of more than one, or at most two, one-hundreths of a second, the pendulum of the apparatus being under the control of the clock in the way above mentioned.

I shall assume then that we have an axis revolving once per second with a uniform motion. It is immaterial, of course, whether this uniform motion is obtained through the spring-governor, or by means of some other of the many ingenious inventions that have been contrived for the purpose, though I do not think any can be found more simple and effective.

Suppose now that in fig. 1, A is such an axis, and that B is a second axis mounted on the prolongation of A, but entirely separate, bearing a type-wheel, *t t*, at its extremity. Suppose also that *f* is a balanced arm attached to the extremity of A, and that *b* is a similar arm attached to B, and bearing a pin, *d*, which engages with *f*.

1.



It is then evident that A in its motion would take along the type-wheel, *t t*, in precise coincidence with itself. If now, at the instant of observation, it were possible to remove the pin *d*, the type-wheel might be stopped, an impression be obtained of whatever figures were uppermost, and then, the pin being replaced, at the next revolution the arm *f* would encounter it and again put the type-wheel in motion as before.

By this operation the arbor A would have been subjected to the two following disturbances: 1st, during one second (for the time of making the imprint need never exceed a fraction of a second) it would be relieved of whatever friction might be due to the revolution of B. This relief from friction would tend to acceleration: 2d, at the instant when the arm *f* strikes the pin it will receive a shock due to the inertia of B and its appendages; this would tend to retardation. The difference only of the two effects would have to be corrected by the action of the spring-governor; and it is believed, if the governor train has considerable momentum, and the type-wheel *t t* is made as light as possible, that the disturbance will be nearly or quite insensible.

The mechanical arrangements by which this idea is carried out are more fully represented in figure 2, which is drawn of the actual dimensions proposed. It represents a vertical section through the plane of the seconds-axis A. This is made to project about half an inch through the front plate of the chronograph frame, and carries a spiral cam, *a*, and an arm, *b*. The cam acts upon the end of a lever (not shown in the cut) which, by a ratchet movement, causes the wheel L, near the right of the figure, to advance one step at the end of every second.

The type-wheel K, which indicates the seconds, is connected with L by a spring. This spring comes into action only when the hammer happens to be down on the face of K (in the act of printing an observation) at the same instant that the lever, dropping off from the end of the spiral *a*, is urging the wheel forward. Were it not for the intermediate wheel L and the connecting spring, a second might be lost in the indication of the type-wheel K on such an occasion. As it is, the wheel L will move on to its proper place, bending the spring a little, and as soon as the hammer rises K will also be carried forward by the spring.

A cam, M M, on the wheel L drives the minute-wheel N by a similar lever and ratchet; and the hour-wheel, if it is thought worth while to have one, is to be driven from the minute-wheel in the same way.

It would be better perhaps to relieve the arbor A from the work of driving the second and minute wheels, by using for the purpose a separate clock movement controlled magnetically by the standard clock, as proposed by Prof. Hough; but this would involve considerable additional expense.

The type-wheel *t t* is mounted upon its axis B B, in the prolongation of A, its bearings being ivory boxes, represented at *h* and *h'*. This type-wheel is made as light as possible; the vertical portion is of steel as thin as is consistent with strength, while the rim is a thin strip of copper soldered to the steel disc, and bearing in raised type the figures 00, 02, 04, 06, &c., up to 98. The copper strip may be made by the electrotype process from a leaden matrix in which the figures have been sunk at proper intervals by a common type punch.

As this rim would be hardly stiff enough to print from without some support, the bearing *h* is held to its place by a spring little more than strong enough to support the weight of the wheel; when the hammer descends to make an imprint this yields a little, and allows the rim of the wheel to come down upon the bed F.

The wheel *t t* is adjustable upon its axis so that its zero can be made to come uppermost at the instant when the wheel K effects the change of the indicated second.

The type-wheels K and N are constructed in the same way, only their rims are made heavy enough to print without any support except that afforded by the disc of the wheel.

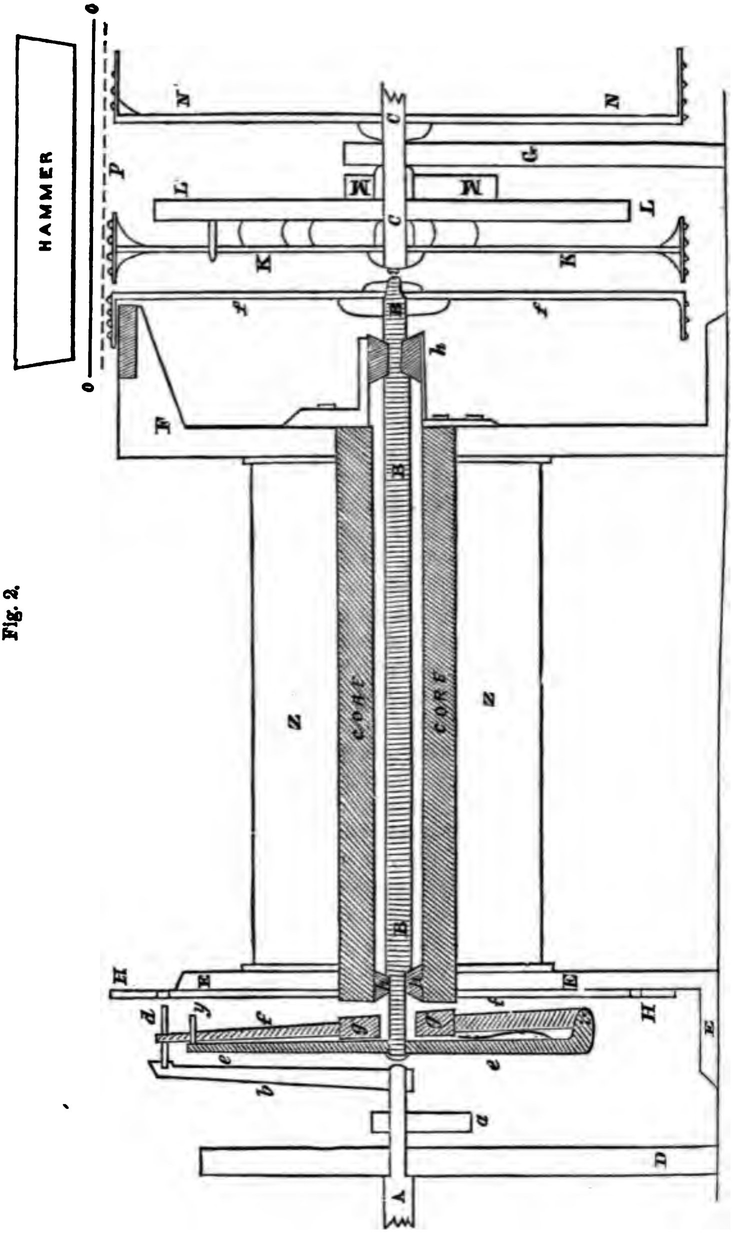


Fig. 2.

The arm *e e* is firmly secured to the left-hand extremity of the axis B, and carries at one end two pins (*y*) near each other, orming a fork; at the other end it has a little projection (x). The arm *ff* is delicately pivoted at (x), and at the other extremity passing freely between the pins of the fork (y) carries the steel pin *d*. At the middle it is expanded into a ring, *gg*, through which the arbor B passes without touching: *ff* is made of soft iron, and the annular expansion at the middle thus serves for the armature of the electro-magnet whose action is to produce the desired result at the touch of the observer's key.

A light spring between *e* and *f* solicits *f* to the left, and thus whenever the electro-magnet is not acting keeps the pin *d* in the position represented in fig. 2, engaged with the arm *b*. In this state of things the type-wheel *tt* will be carried round continuously by A. Of course the arms *e* and *f* must be as light as consistent with sufficient stiffness, and their weight must balance in all parts of the revolution, and neither tend to accelerate or retard the motion of the type-wheel.

Close behind the arms *e* and *f* is placed a stationary disc of metal, H H, having one hundred equidistant holes pierced in it in a circle, so situated that whenever the arm *f* is drawn to the right by the magnet (thus disengaging the type-wheel from the train) the pin *d* will immediately enter one of these holes, stop the type-wheel, and hold it in place until the magnet ceases to act.

This magnet is peculiar in having for its core, instead of a solid rod, an iron tube, through which the axis B passes. Thus the pole of the magnet is always in the same position with reference to the armature *gg*, notwithstanding its revolution. As only one pole of the magnet acts upon the armature it is necessary to make the coils more powerful than ordinary.

For bringing the hammer down upon the paper and moving the paper along after each impression any one of many different plans might be used. Probably the best, leaving expense out of the account, would be to have the hammer raised by an independent train of wheelwork, which should be unlocked by an electro-magnet at the instant of observation, thus releasing the hammer, and allowing the wheelwork after the blow to move far enough to raise the hammer again, and carry the paper forward.

Another more simple plan is to work the hammer directly by a powerful electro-magnet, to which the magnet Z should act as a relay—that is, whenever the pin *d* touches the disc H it should establish a current which should bring down the hammer upon the paper; the hammer in rising after the blow carries the paper along one space.

Provision is also made for carrying the paper along several spaces at the will of the observer, so as to leave an interval between the record of different stars.

Although there are only fifty numbers on the type-wheel which prints the decimal of the second, the record is made to the nearest hundredth. There being one hundred holes in the disc H, the type which indicates the decimal of the second may either come in line with that which gives the whole seconds, or half a space above it, thus: 25·18 or $25^{\cdot 18}$; the first would be read twenty-five and eighteen hundredths, the second twenty-five and nineteen hundredths.

It is not intended to secure precise coincidence of *error* between the clock and chronograph—merely coincidence of *rate.* This is obtained by controlling the pendulum of the spring-governor from the clock. The type-wheels can be set so as to indicate the nearest whole second; and then the exact difference between the clock error and the chronograph error can easily be found by making the clock record itself occasionally at the beginning of a minute.

The operation of the instrument is then as follows. When the observer touches his key, the magnet Z acts upon the armature, and withdraws the pin *d* from its engagement with *b*, causing it to plunge into one of the hundred holes in the disc H. The contact of *d* with H in its turn, by a magnet not shown in the cut, brings down the hammer upon the paper *o o* and forces it against the type, a piece of impression paper being interposed.

When the observer takes his finger from the key, *d* returns to its original position and will engage with *b* at its next revolution. The hammer also rises, and in rising carries the paper along one space in readiness for the next impression.

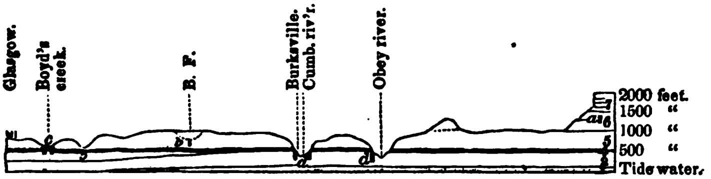
As yet the printing chronograph exists only as an idea, but it is hoped that the idea will soon be realized, and the machine put in operation at the Shattuck Observatory. The result of the experiment will form the subject of a future communication.

Dartmouth College, April, 1866.

ART. XVIII.—*Note on the geological position of Petroleum Reservoirs in Southern Kentucky and in Tennessee;* by Prof. J. M. SAFFORD.

THE object of the following note is to point out briefly the geological position of the petroleum reservoirs in Southern Kentucky and in Tennessee, so far as they have been met with within the field of my observations. I hope, in a future article, to give a summary of all ascertained facts with reference to the mineral oils of this region.

The accompanying general section will serve to illustrate the topographical and geological features of the region under consideration. The line of section extends from the Cumberland mountain, or table-land, in Putnam county, Tennessee, through Overton county, in a direction a little west of north, to Burksville, Kentucky, and thence to Glasgow. The entire distance is about seventy miles. The region traversed by this line is a plateau, from 800 to 1000 feet above the sea, out of the nearly horizontal strata of which the larger streams have eroded valleys from 300 to 500 feet deep.



The following are the formations represented in the section:

7. *Coal-measures*, 400 ft., edge of table-land.

6. *Mountain Limestone*,[1] about 550 ft. thick in Putnam county; mostly limestones.

5. *Siliceous group;* the "knobstones" of Kentucky. From 300 to 500 feet thick, including the Lithostrotion beds as its upper part.

4. *Black slate*, Devonian and Genesee, having a maximum thickness of about 60 feet.

3. *Upper Silurian* mostly or wholly, from 100 to 150 feet thick; mostly a series of limestones, some of which are impure, approaching fine sandstone or shale in character. The existence of these strata in the region of Glasgow is mainly *inferred* from the fact that they are seen in certain sections to the northeast and southwest of this point. They are, however, comparatively unimportant, and thin out southeastward and disappear.

2. *Nashville group*, Mr. Dana's Hudson period; blue fossiliferous limestones with some calcareous shales, 500 feet.

1. *Trenton limestones*, at the base of the section.

It may be remarked, in passing, that one of the most striking features of this section is the almost entire absence of the Upper Silurian and Devonian formations. In the Tennessee and Cumberland river portions, the Upper Silurian beds are wholly wanting, while the Devonian series is represented by nothing more than the thin Black Slate—a fact pointed out by me many years ago.

I have represented in the section the geological places of what we may call typical petroleum wells by the short heavy vertical lines. We will notice them in descending order.

1st. *In the Mountain Limestone.* The heavy line at *a* in the upper part of this formation indicates simply the geological level of the "Beaty oil well." Its geographical and topographical positions are very different. The well is located in Kentucky on the Big South Fork of Cumberland river, and near the Tennessee line. The valley of the stream is, at the well, narrow, and is deeply set in the Cumberland table-land; it cuts through the Coal-measures and into the top part of the Mountain limestone, exposing about 50 feet of the latter. The Mountain limestone is therefore much depressed at this point. The well is sunk near the river, and is less than 200 feet deep. It was bored, about 1825, for salt water. At its greatest depth, a reservoir of oil was struck, from which so much petroleum flowed as to lead to the abandonment of the boring as a salt well. For several years after petroleum was gathered at this point for medicinal purposes. How much petroleum issued from this boring it is now impossible to tell. I give it simply as a good example of an oil reservoir actually tapped in the Mountain limestone.

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[1] The awkward term *subcarboniferous* ought to be dropped. Silurian rocks are subcarboniferous.

2d. *In the Siliceous group.* There are several examples in this formation of reservoirs reached by boring. At *b* in the section the geological (not geographical) place of the "Porter well" in Allen county, Kentucky, is represented. This well is located on Bay's Fork of Big Barren, on a line between Scottsville and Bowling Green, and about seven miles from the former place and eighteen from the latter. This reservoir was tapped, some time in January of this year, at the moderate depth of 55 feet. It yielded for a number of days, by pumping, about 400 barrels of oil and strong brine per day, half of which was oil. At the time of my visit, Feb. 13th, it had produced altogether about 1000 barrels of petroleum, but was not then doing well.

In the southern part of Overton county, Tennessee, on Spring creek, is another example. Here a reservoir was struck which yielded heavy oil, but how much I am not informed.

3d. *In the Black Slate.* On Boyd's creek, near Glasgow, Kentucky, is a group of half a dozen wells or more. Their position is shown by the heavy vertical lines at *c*. One or two of these met with oil in the Black Slate.

4th. *In the Upper Silurian.* All of the Boyd's creek wells start in the Siliceous group, most of them pass through the Black Slate and terminate in this group. Taking all the wells, they vary in depth from 60 to 250 feet, averaging about 130 feet. So it was at least at the time of my visit. One of them was, and may be now, a flowing well, having yielded for many months nearly or quite 30 barrels of oil per day. One or two of these borings may reach the next group of limestones below. In fact practically, and possibly in reality, the rocks may be united with the Nashville group.

5th. *In the Nashville group.* This group has furnished the most and the largest reservoirs. The geological and topographical place of a number of borings, which have tapped oil reservoirs, on the Cumberland and Obey rivers, and on their tributaries, both in Kentucky and Tennessee, is shown by the heavy t lines at *d*, *d*. The old "American oil well" near Burks- ;, originally bored for salt water, may be taken as an exam- This, from top to bottom, is within the Nashville group. nouth is not far from 40 feet below the level of the Black э. At the depth of about 175 feet this boring tapped an oil rvoir, from which flowed out, at a minimum estimate, 50,000 els of oil. The recently bored Crocus creek well, which yielded up to this time not far from 30,000 barrels, is an- r example. Other examples might be given, but these are cient. A great number of wells will be sunk in this forma- during the present year.

h. *In the Trenton limestones.* Some boring has been done in series, but as yet no repositories of any note have been dis- ›red, at least in Tennessee.

shville, Tenn., March 19th, 1866.

'. XIX.—*Analyses of some minerals from the Emery mine of hester, Mass.;* communicated by Dr. C. T. Jackson. (From letter to one of the Editors.)

Andesine.—The emery vein enlarges as it goes in, and from feet has already widened to seven feet eight inches of solid ry of the best quality. The adit now is extended 260 feet. portion of rock originally mistaken by me for granular rtzite, and called Indianite by Shepard, proves on analysis to *Andesine*, although it is harder than stated in the books, tching quartz crystal readily. It is associated with crystals ɔlack tourmaline. It is very compact, fine granular in ure, with a conchoidal splintery fracture, and has G. = 2·586, = 7·5; the color slightly greenish white. I obtained for its position,

	1.	2.
Silica,	62·00	60·00
Alumina,	24·40	25·00
Lime,	3·50	
Magnesia,	0·70	
Soda,	8·07	
Water,	1·00	
	99·67	

n No. 2 there was a trace of oxyd of iron not weighable.

. *Analysis of Margarite*, by John C. Jackson.—The margarite Chester has, G. = 3·03, H. = 3·5–4. The analysis afforded:

Silica,	29·84
Alumina,	53·84
Lime,	10·38
Magnesia,	0·24
Alkalies, soda chiefly,	2·46
Water,	1·32
Sesquioxyd of iron,	0·30
	98·38

3. *Diaspore.*—The first of the following analyses of diaspore was made by my son, John C. Jackson. I was in hopes he would have had time to repeat the work and determine the alumina directly,—an accident having damaged that part of his analysis so that he can give it only by difference. In my analysis of the same mineral the ingredients were all directly determined. The quantity analyzed at a time was 10 grains; two analyses were made. The diaspore is in prismatic crystals which contain microscopic crystals of Brookite. H. =7 nearly, scratching quartz distinctly but feebly. G. = 3·39. Analyses:

	1.	2.
Water,	14·75	14·8
Alumina,	[80·75]	83·0
Oxyds titanium and iron,	4·50	
Sesquioxyd of iron and oxyd titanium,		3·0
	100·00	100·8

The diaspore occurs in both the North and South Mountains, associated with emery and chloritoid. It exists both in bladed striated crystals, and in small prisms of considerable length, sometimes an inch or more long. Only the microscopic crystals present perfectly defined forms.

Chloritoid.—Ten grains of the chloritoid were selected for the analysis, as pure as possible, but it still contained microscopic particles of magnetic iron ore and perhaps of emery.

The results of my analyses are as follows. The second column contains these results as they would be if half the oxyd of iron is protoxyd:

	1.	2.
Water,	11·00	11·00
Silica,	22·50	22·50
Alumina,	23·50	23·50
Protoxyd of iron,		18·00
Sesquioxyd of iron,	41·50	20·25
Magnesia,	1·80	1·80
	100·30	97·05

If I can procure crystals free from any admixture I shall re-analyze it. It is plain that if the mineral is chlorite the magnesia is replaced by protoxyd of iron. It differs less from chloritoid but more from masonite, which by my analysis, published in my Report on the Geology of Rhode Island, contains 6 pr. ct. of oxyd of manganese and 32·20 of silica. The following is my analysis of masonite: (From analyses made in 1839 and published in 1840; Geol. of R. I., page 88, Prov., R. I., 1840. The analysis was repeated several times, and this is a mean of a number of carefully made analyses by myself on 25 and 50 grain lots.)

Water,	4·000
Silica	33·200
Alumina,	29·000
Magnesia,	0·240
Protoxyd of iron,	25·934
Oxyd of manganese,	6·000
	99·374

Boston, March, 1866.

ART. XX.—*On the detection of Iodine;* by M. CAREY LEA.

WHERE iodine exists in the form of hydriodic acid, or the iodid of a base, two methods are commonly employed to put it into a condition to be detected by the starch test. One of these is by the action of nitric acid, the other by chlorine or bromine water. The latter is the more delicate, but has the disadvantage that if the chlorine or bromine be added in excess, the reaction is missed.

It occurred to me while engaged in testing for iodine, that the facility with which that body is eliminated from its hydrogen and metallic combinations by *chromic acid* would make the latter substance a valuable means of bringing about the starch reaction, and a few experiments completely confirmed this view.

If, for example, we take an extremely dilute solution of iodid of potassium, such that the addition of nitric acid and starch produces no perceptible effect, the further addition of a single drop of very dilute solution of bichromate of potash will instantly bring about the characteristic reaction.

When chlorhydric acid is substituted for nitric, the effect of the bichromate is (as was to be expected) still more marked. The test has then the full delicacy at least of the chlorine test, with this great advantage, that an excess of the reagent does not prevent the reaction.

As to the delicacy of this test, the following observations were made.

With solutions of iodid of potassium up to one hundred thousandth (1 : 100,000) the precipitate was abundant, becoming less blue and more tawny as the dilution increased. Beyond this point the distinctness rapidly fell off. The indications were observable at one-four-hundred-thousandth. With a solution of one-eight-hundred-thousandth it was doubtful whether any effect was evident though still it was thought that a darkening was produced.

The experiment can be made in two ways, according to the result desired.

If it is wished to observe the effect of the chromic acid in increasing the delicacy of the indication, add the acid and starch to the very dilute solution of iodid, and then when the extreme dilution is such that no reaction appears, a drop of solution of bichromate instantly produces it.

But in employing the reagent in the search for iodine, add the starch to the liquid to be tested, stir it up, add a drop of dilute solution of bichromate, enough to communicate a pale yellow color, and finally add a few drops of chlorhydric acid. The test is then the production of the characteristic precipitate, or in case of great dilution, approaching to a half-millionth, merely a tawny shade given to the solution.

It seems scarcely necessary to say that if a very great excess of acid is used, and too much bichromate, the starch may be made to reduce the bichromate. Even this, however, cannot deceive, for a bluish-green solution is thereby produced, whereas the indications of iodid are in the order of their strength: blue precipitate, tawny precipitate, tawny solution. Unless in the case of very exceptional dilution above spoken of, a well marked blue precipitate is always obtained.

The examination of the delicacy of the reaction with very dilute solutions was made at a temperature of 65° F. or thereabouts. This fact requires to be taken into account, as according to some experiments of Fresenius to be found in the Jahresbericht for 1857, the delicacy of the starch test increases as the temperature falls, so that at 0° C. a fainter trace can be rendered evident than at 12° C., and so on: the difference is asserted to be material. Fresenius's experiments were made with sulphuric acid and hyponitric acid, and the delicacy of the reaction obtained by him at corresponding temperatures seems to fall a little short of the above.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the preparation of Hydrofluoric Acid ;* by W. P. Dexter.—Few chemists have at their disposal a distillatory apparatus of platinum, and the cryolite from which the purest hydrofluoric acid is prepared; and the shape commonly given to the dome of platinum retorts is such as to allow matter which may be projected upon it to flow down the neck into the receiver. The acid made from fluor spar in such a retort I have found to contain sulphate of lime. A simple remedy for this defect would be to fix in the dome a perforated disc, or ring, over the aperture of which another disc of less diameter, but larger than this aperture, is supported by three strips of platinum rivetted to the ring, both being made of silver platinum foil.

A dome of platinum attached to a leaden vessel seems to me a half-way measure, combining the disadvantages attending the use of both metals.

To those who are not in possession of an apparatus of platinum, I can recommend from experience the following comparatively inexpensive arrangement.

It consists of the ordinary leaden bore, (mine is 6″ high by 3½″ internal diameter,) made of a piece of lead pipe into which a bottom of lead is cast, and provided near the top with a small and short tube for the escape of the gas. The tube must incline slightly from the retort up-

ward, otherwise whatever is condensed or projected upon it will flow downward and the product be contaminated, at least, with lead. Into this tube a smaller one of platinum is luted, which is bent into the shape of a quarter of a circle so that the farther end points downward; this end is soldered with gold into the bottom of an inverted platinum crucible. An old one, perforated and cracked, such as is generally to be found in a laboratory, answers perfectly for the purpose.

The inverted crucible hanging like a bell at the end of the tube, when immersed in water contained in a vessel not very much exceeding it in diameter, offers a large surface for the absorption of the acid gas, while a retrocession of the liquid from absorption or change of temperature is impossible.

The cover to the retort may be cast in one piece with a shoulder, or be made of two discs of lead of the size of the inner and outer diameters of the bore, and held together by a ring of lead cast into them and serving as a handle. For a lute I spread a thin layer of gypsum on the surfaces in contact, and cover the joint on the outside with a paste of rye-meal.

2. *Skylight Polarization at Philadelphia;* by PLINY EARLE CHASE, A.M., S.P.A.S.[1]—Recent observations with a Savart polariscope having led me to results which, while generally confirmatory, differ in a few particulars from those published by Sir David Brewster, (Phil. Mag. [4], xxx, pp. 118, 166, sqq.), I place some of them on record, to facilitate a comparison with similar observations at other places.

(1.) In all the great circles which pass through the sun, the polarization of a clear sky is positive, except in the neighborhood of the solar and anti-solar points. If the polariscope is rotated from the positive maximum, the bands gradually diminish in brilliancy, vanishing at about 45°, and attaining a negative maximum at about 90°.

(2.) Within the primary lemniscates, of which the solar and anti-solar points are the respective centers, and the neutral points (actual or theoretical)[2] are the limits; the polarization of a clear sky is negative when the bands pass toward the sun's center, vanishing when the bands are inclined 45° to the solar radii, and attaining a positive maximum when the inclination reaches 90°.

(3.) Arago's and Babinet's neutral points can be seen as well before sunrise as after sunset, provided the atmospheric conditions are the same. Brewster gives the preference to the evening observations, but apparently for no other reason than that the sky is then usually clearer than in the morning, (op. cit., p. 118.)

(4.) I have repeatedly, and with little comparative difficulty, observed Brewster's neutral point. In the majority of cases, when the sun's altitude has been sufficient, I have been able to fix its position with nearly as much facility as that of Babinet's. (For the difficulties of Brewster and Babinet, see loc. cit., pp. 119, 166, 181.)

(5.) Within the solar primary lemniscate it is frequently difficult to make any ordinary observation of the polarized bands, on account of the dazzling intensity of the light. But when the direct rays of the sun have been shut off by a thin disc (placed with its edge towards the eye, so that the polarization will not be affected by reflection from the surface of the disc), I have often been able to mark the opposite polarizations and the position of the neutral points with perfect ease, even at mid-day.

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[1] From the Proceedings of the American Philosophical Society, Jan. 5, 1866.

[2] There is one *theoretical* neutral point below the anti-solar point. It is probably never above the horizon when there is light enough to determine its position.

(6.) In our climate it is by no means unusual to have days on which all the three neutral points can be observed, and their places determined. During the whole period of Brewster's observations at St. Andrews, he found but two such days, April 5th and 8th, 1842, (loc. cit., pp. 124, 163).

(7.) Quasi-neutral lines, dividing bands of opposite polarization, can be found in nearly all parts of the sky by rotating the polariscope 45° from the line of maximum positive or negative polarization. But a slight additional rotation will show that the neutralization is only apparent.

(8.) The position of a true neutral point can be determined by sweeping its neighborhood alternately with the vertical and with the horizontal bands, and marking the intersection of the lines of vanishing polarization.

(9.) In consequence of the arrangement of the lines of equal polarization, when the sky is swept with the polariscope for a few degrees on each side of a neutral point, the line which separates the oppositely polarized bands forms curves with a convexity determined by the position of the sun or of the anti-solar point.[*]

(10.) Some of my observations have indicated an apparent correlation between these curves and the magnetic dip and terrestrial latitude. I have not been able to satisfy myself whether the correspondence was merely accidental, or whether it indicated another point of analogy between the laws of light and of magnetism.

(11.) The varying effects of haze and cloud, appear, on the whole, to sustain Brewster's theory, that the neutral point is produced "by the opposite action of light polarized by reflexion and refraction." (See pp. 123, 169, 176, 178, 180.)

(12.) In one instance, soon after sunset, the reflection from scattered clouds in the neighborhood of the anti-solar point was such as to totally eclipse Arago's neutral point, the polarization being positive over the entire arch, from Babinet's neutral point to the eastern horizon.

3. *Comparative visibility of Arago's, Babinet's, and Brewster's Neutral Points;*[1] by PLINY EARLE CHASE, M.A., S.P.A.S.—In my communication of January 5th, I stated that when Brewster's neutral point is above the horizon, I had frequently determined its position with great ease. My experience was so different from those of Brewster and Babinet, that I commenced on the 6th of March a series of comparative observations upon the three neutral points. The month which has just ended appears to warrant the following conclusions for stations in Philadelphia and its vicinity. The 1st, 2d, and 6th seem to be confirmed by observations elsewhere, while the 3d, 4th, and 5th, which are, perhaps, dependent partly upon local atmospheric peculiarities, have never, so far as I am aware, hitherto been noticed.

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[*] I am not sure whether this is the "singular effect" thus described by Brewster (loc. cit., p. 124); "In conveying the bands vertically round, the neutral line, in place of crossing them at a right angle, was the arc of a circle, to which one of the bands was a tangent." (See, also, pp. 121, 167.)

[1] From the Proceedings of the American Philosophical Society, April 6, 1866.

(1.) Arago's neutral point often assumes a distinctness which is never exhibited by either of the others, merely because the polarized bands in the vicinity of the sun are obscured by the dazzling brilliancy of its rays.

(2.) For the same reason, Babinet's neutral point is often better defined, in the morning and evening, than Brewster's during the middle of the day.

(3.) But when Brewster's and Babinet's neutral points are both above the horizon, if the sky is clear, the former is generally *more easily* posited than the latter. This is especially the case at midday.

(4.) On every clear day, and on a large portion of the days which are partially obscured by clouds, the position of each of the neutral points can be determined. Brewster records but two days during five years' observations (Phil. Mag. [4] 30, 124), upon which he saw all the points.

(5.) Arago's neutral point often rises before Brewster's sets. Under favorable atmospheric conditions the three points are, therefore, sometimes simultaneously visible.

(6.) Halos and clouds are frequently discernible through the polariscope, which are invisible to the naked eye.

The following abstract embodies some of the results of the month's observations:

Satisfactory observations were made on	25 days.
All the neutral points were seen on	17 "
There were no satisfactory observations on	6 "
" " 39 observations of Arago's neutral point on	23 "
" " 93 " " Babinet's " "	22 "
" " 59 " " Brewster's " "	20 "
Arago's neutral point was remarkably distinct on	4 "
Babinet's " " "	10 "
Brewster's " " "	11 "
Arago's was the only one observed on	2 "
Babinet's " " "	1 day.
Babinet's and Brewster's the only ones seen on	1 "
Arago's and Babinet's " "	2 days.

The three points were simultaneously visible on April 5th, from $4^h\ 32'$ to $4^h\ 42'$ P. M.

Brewster's neutral point was perceptibly more than Babinet's at fifteen observations, and less distinct at two observations.

I subjoin a few of my notes, which refer to points of special interest: March 8th, $5^h\ 45'$, P. M. Near the proper position for Arago's neutral point, the positive and negative polarities coalesce upon clouds, with no intervening space or neutral line.

March 9th, $6^h\ 25'$, A. M. Hazy and polarization fluctuating. $10^h\ 40'$, A. M. The polariscope showed a brilliant halo around the sun, which I had not before noticed, but which was afterwards barely visible to the naked eye. $12^h\ 10'$, P. M. Haze continues. Negative polarity remarkably distinct over the face of the sun, and for several degrees North and South.

March 11th, $3^h\ 50'$, P. M. Sky covered with thin clouds. A neutral point in the East, 42° above the horizon, and *more than 70° from the anti-solar point*, with reversed polarization, or positive below, and negative above. $5^h 25'$. A similar point still observable, but about 5° nearer the horizon.

March 12th, $6^h 30'$, A.M. Cloudy. Polarization positive from East and West horizon, nearly to Zenith. A similar observation was made March 21st, at 6^h P.M.

March 17th, $9^h 15'$ and $10^h 40'$ A.M.,[2] and March 18, $10^h 30'$, A.M.[3] Very clear. Sun so bright that I was unable to detect the negative polarity between Babinet's neutral point and Brewster's, even by screening the eye from the direct light of the sun.

March 19th, $11^h 5'$, A.M.[4] Halo, visible only through the polariscope. 1^h P.M. Snowing.

March 20th, $5^h 25'$, P.M. Cloudy. Polarization in horizon everywhere positive.

March 24th to 28th, inclusive. On each of these five successive days Brewster's neutral point was remarkably distinct and beautiful.

April 3d, $5^h 40'$ P.M. Cloudy in West, and polarization positive from zenith to horizon.

Strong reflection sometimes changes the character of a comparatively weak polarization, from positive to negative, or *vice versâ*. A fainter reflection, by showing whether the bands are interrupted or continuous, often aids in determining the character of the polarization.

The increased refraction of a piece of glass, interposed between the polariscope and the sky, will frequently show a neutral point which is otherwise invisible.

The normal polarity is often reversed by a stratum of clouds of uniform thickness, especially within the solar primary lemniscate.

II. MINERALOGY AND GEOLOGY.

1. *On the age of the gold-bearing rocks of the Pacific Coast*; by Prof. WM. H. BREWER. (Communicated for this Journal).—In the preceding number of this Journal, in a *résumé* of Whitney's "Geology of California," I noticed in some detail (pp. 361–364,) the principal data from which the secondary age of the auriferous rocks of the Pacific Coast had been deduced, with the dates of the more important discoveries, and of their first publication. These included a reference to the more important fossils that had come under the observation of the members of the Geological Survey up to the time of Prof. Whitney's first publication, together with some additional facts, confirming the conclusions that had been discovered later.

Since that article was printed, a paper bearing about the same date has been received, entitled,

"Annotated Catalogue of the Principal Mineral Species hitherto recognized in California and the adjoining States and Territories; being a Report to the California State Board of Agriculture, by WM. P. BLAKE, Geologist of the California State Board of Agriculture, and Professor of Mineralogy, Geology, and Mining, in the Department of Science of the College of California. Sacramento, March, 1866.

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[2] On steamboat in Raritan Bay.

[3] At Eagleswood, near Perth Amboy.

[4] In New York.

In addition to the Catalogue of Minerals it contains about four pages of, "Notes on the Geographical Distribution and Geology of the Precious Metals" "on the Pacific slope."

These "Notes" contain statements apparently so entirely at variance with the facts I detailed in the *résumé* mentioned, that, unless answered, they are not only calculated to mislead those who are interested in the history of geological discovery in California, but also to call in question the authenticity of some of those facts; as well as the statements relating to them in the various publications of the California geological survey. The official character of this document gives the remarkable statements and claims it puts forth their principal weight, and demands that they be carefully examined, particularly as regards those differences which exist between these statements on the one side, and those published in Prof. Whitney's Reports and papers on the other. This is especially important as it relates to the question, *who first demonstrated and first published the Secondary age of the auriferous rocks of California.* Prof. Blake says:

> "After years of laborious search for fossils by which the age of the gold-bearing rocks might be determined, I had the pleasure, early in 1863, to obtain a specimen containing *Ammonites*, from a locality on the American River, preserved in the cabinet of Mr. Spear. This fossil was of extreme importance, being indicative of the Secondary age of the gold-bearing slates, and was therefore photographed, and copies of it sent to the Smithsonian Institution at Washington, for description. It was subsequently noticed in the Proceedings of the California Academy of Natural Sciences, Sept. 1864." (Page 28 of pamphlet.)

We might infer from this, (1,) that the fossil spoken of was found *in place*, (for similar fossils found not in place had been known several years earlier); (2,) that it was sufficiently well preserved to be determined, and even from a photograph; and (3,) that its secondary age was published in Sept. 1864. I find on referring to his original paper, (Proc. Acad. Nat. Sci., iii, p. 167, which was not published until *December*, 1864,) the following additional information: "It is not certain whether the specimen was taken from the slates in place, or broken from a loose mass." "It is not possible to determine from the specimen whether these fossils are new or not, *or even whether they are ammonites or ceratites.*" This announcement was made a year (and it was not published until three months later) after the Geological Survey had taken nearly twenty recognizable species of Jurassic and Triassic fossils from the auriferous slates, and also later than the publication of Prof. Whitney's announcement in this Journal of the Secondary age of the formation. I leave out of consideration the Carboniferous fossils found in limestones enclosed in the true slates, near Pence Ranch, in 1862, and the other discoveries of fossils before Sept. 1864, which are noticed in the Report on the Geology of California.

He observes again:

> "The same year, when at Bear Valley, Mariposa county, upon the chief gold-bearing rocks of California, I identified a group of Secondary fossils from the slates contiguous to the Pine Tree Vein, and noticed them at a meeting of the California Academy, Oct. 3, 1864, announcing the Jurassic or Cretaceous age of these slates. The best characterized fossil was a *Plagiostoma*," &c. (Ib.)

These fossils were *not* found in 1863, as the language implies, but late in Sept. 1864. On referring to his original paper in the Proceedings of the Academy, (iii, p. 170), I find that he "identified" the fossils by referring them all to wrong genera, (as determined by Mr. Meek).

Again he states:

> "The attention of the Geological Survey having been directed to this locality by my announcement and exhibition of fossils in San Francisco," &c. (Ib.)

The attention of Mr. Gabb and myself was directed to those particular species by the announcement; but Mr. King had already procured and forwarded to Prof. Whiting, for description by Mr. Meek, similar specimens from the same locality before Prof. Blake had seen or heard of a fossil being found there. I had not been advised of Mr. King's action in the matter, nor had Mr. Gabb, who afterwards visited the locality and obtained more specimens.

He remarks further:

> "It appears also, from the same source, (Whitney's Geology of California), that Mr. King, a gentleman connected with the Survey, had obtained *Belemnites* from the Mariposa rocks in 1864," &c. (Ib.)

Prof. Blake neglects to add that the same source informs him that these Belemnites were found *in place* very near to Pine Tree vein, and *eight months before* the fossils mentioned in the preceding paragraph had been found.

Again:

> "The Silurian age of the gold rocks of California has not always been assumed. It has been repeatedly questioned. In the preface to the writer's 'Report of a Geological Reconnoisance in California' it is stated that a considerable part of the gold-bearing slates of California are probably Carboniferous." (p. 29.)

I find on referring to that preface by him that the next sentence to the one he cites is as follows: "It is also probable that a great part of the rock formations of the gold region will ultimately be found to be Devonian or Silurian," &c., and in the later pages of the same work he paves the way for priority of discovery, should they actually prove to be Silurian, by stating that the conclusions arrived at by Sir R. I. Murchison were confirmed by his (Prof. Blake's) observations in California.

Again:

> "The opinion of the comparatively modern age of the gold rocks has been steadily gaining strength for years past, and has been the subject of discussion in the daily journals."

As he does not cite his authorities in this case, I cannot positively deny that such *surmises* may have been printed, but I never saw them until *after* Sept. 1864, since which date certain writers have recollected that they had arrived at these conclusions, and are now putting forth claims for their "discoveries."

I maintain that Professor Whitney's article in the American Journal of Science, Sept. 1864, is *the first published statement of the fact*, that "the sedimentary portion of the great metalliferous belt of the Pacific coast of North America is chiefly made up of rocks of Jurassic and Triassic age, with a comparatively small development of Carboniferous limestone," (Am. Jour. Sci., xxxviii, p. 261). This was widely circulated, and was even reprinted in Europe, before Prof. Blake's "announcement," and some months before said announcement was printed. (See London Mining and Smelting Magazine, Oct. 1864, pp. 215–217).

And, furthermore, the conclusions were reiterated in the preface to the Paleontology (vol. i, p. 18,) which was issued in Dec. 1864, the same month with Prof. Blake's paper, and this in turn received notice in this Journal for Jan. 1865, p. 99. In this volume were described over 50 species of fossils of the age under consideration, more than half of which had been found in California in the rocks associated with gold. Many of the plates and descriptions of these fossils had been prepared more than a year before this, or in 1863.

Prof. Blake adds:

> "I regret to observe that in this publication (Whitney's Geology of California), as well as Mr. Gabb's notice of the fossils, no mention is made of my previous announcement, and that my part in the discovery and publication of the Secondary age of the Mariposa gold rocks is studiously and wholly ignored." (Foot-note to p. 28.)

While the language quoted only strictly claims a part in the "discovery and publication of the Secondary age of the *Mariposa* gold rocks," yet any person not acquainted with the facts, and not examining the dates of the original discoveries and publications, would draw the inference from the connection in which the statement stands, that Prof. Blake had been the first to discover and announce the age of these rocks, (if not of the gold series of the State as a whole). But I have already shown that, even as applied to the Mariposa rocks, the claim is unfounded. Mr. King antedated him eight months in the discovery of fossils, and Prof. Whiting three months in publishing the conclusions.

Nor could Prof. Blake have been ignorant of this, for he had all the printed data in the Reports he cites.

The article under review being an official Report published by the State Board of Agriculture in the Transactions of the State Agricultural Society, as well as in pamphlet form, is intended to reach the more intelligent portion of the people of that State, to diffuse reliable information among them, to influence their future policy in official surveys, as well as "to help to arouse an interest in the science of mineralogy among our people." The natural inference drawn from the language and bearing of this part of the document, is, that Prof. Blake, the Geologist of the Board of Agriculture, had made the first discovery and announcement of the true age of the great metalliferous belt of the State, and that the State Geological Survey had robbed him of the honor and ignored his discoveries. No one would suppose after reading it that his "years of laborious search for fossils" had been so poorly rewarded, or that his "part in the discovery and publication of the age of the gold rocks" had occurred so long after the discovery and publication of the fact by the Geological Survey.

For the information of those interested in the question, I will here state that I was in California during the period of the discoveries under consideration, and collected a part of the fossils in the possession of the State Geological Survey, and was acquainted with the localities and dates. I was present at the meeting of the California Academy, Oct. 3d, 1864, when Prof. Blake exhibited his Mariposa fossils and made his so-called "announcement." He prefaced his paper by stating that after years of search for fossils in the gold rocks, by means of which their age might be determined, those which he exhibited were the first he had been able to obtain, and that his attention had been called to these and their locality by Miss Errington. In the verbal discussion that followed the presentation of his paper, I stated the main conclusions arrived at by the Survey, on the subject of the age of the gold rocks. I had already received Prof. Whitney's paper, (which had been published a month earlier), and stated some facts not there detailed in regard to the localities of fossils, and that the Survey had "found fossils in the rocks associated with gold along a line nearly 300 miles in length, extending from Pitt River to the Mariposa Estate," &c. (For synopsis of these remarks, see Proc. Cal. Acad. Nat. Sci., iii, p. 198). I described minutely the Genesee Valley localities for Jurassic, Triassic, and Carboniferous fossils, so that Prof. Blake might find them, as he proposed visiting that region soon. Yet I find in his pamphlet, (p. 28,) the statement, "Fossils of Secondary age from Genesee Valley, in the northern part of the State, *were common in collections in* 1864. (!)

New Haven, June 1st, 1866.

2. *A Catalogue of the Paleozoic Fossils of North America, Part I, Echinodermata;* by B. F. SHUMARD, M.D. 73 pp. 8vo. (From the Transactions of the Academy of Sciences of St. Louis, vol. ii, 1866).—The first signature of Part I, of this Catalogue, to the 13th page inclusive, was issued in the form of extras, in July or August, 1865, and noticed on page 124 of the January number of this Journal for 1866. The succeeding signatures of this part, bear the dates of August and October, 1865, and February, 1866, at which dates extra copies were distributed by the author. Parts 2d, 3d, &c., now in course of preparation, or in the press, will consist of lists of the Plants, *Polyzoa*, *Brachiopoda*, and other groups of North American Paleozoic fossils.

Part I, of this catalogue, now published, and here noticed, is a complete list of the known North American Paleozoic *Echinodermata*. It is not an attempt at a classification of these ancient *Echinodermata*, by arranging them into families or larger groups, in accordance with their zoological affinities, but a simple alphabetical list of species and genera, with full references to the works where they were described or noticed; and as such, it will be a valuable aid to those who may wish to study this class of fossils, since it forms a complete index to the entire literature of the subject. It also gives the geological position of each species, with something of the synonymy, and contains numerous foot-notes of remarks, including descriptions of some of Dr. Troost's previously unpublished genera,[1] and a few new species. At the end of the list, there are likewise tables showing the geological range of the different genera.[2] The whole number of species included is 750, of which 97 are from the

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[1] It is probable that *Cupellæcrinus* Troost, described on p. 361, is not distinct from *Marsupiocrinites* Phillips, (see Murchison's Siluria, p. 219), unless we admit the presence or absence of a proboscis as a distinction.

[2] As it is a pretty well established fact that there are three Archimedes limestones in the Sub-carboniferous series of the Western States, it would have been better if the author had recognized these as distinct rocks, in giving the geological range of genera and species.

Lower Silurian, 86 from the Upper Silurian, 115 from the Devonian, and 452 from the Carboniferous rocks.

In regard to synonymy, in a group like this, including so many closely allied species of which only descriptions have been published, there will of course, be differences of opinion; and beyond the instances where direct comparisons of authentic examples of the allied forms have been made (in cases where no figures have been published), any views on the subject can only be regarded as mere *opinions*, that may be right or may be wrong. Some of the supposed synonyms are believed by the writer of this notice to be distinct species; while he has no doubts whatever in regard to the distinctness of other forms, between which comparisons are suggested. These suggestions, however, will be useful even when the species alluded to are distinct, as hints to those who may have the means to make comparisons, and wish to do so, with the view of studying the relations of allied species.

Although we take great pleasure in bearing witness to the general accuracy and completeness of this valuable catalogue, there are a few points of nomenclature, in which we cannot agree with its author. For instance, in retaining *Elœacrinus* Roemer, 1851, instead of *Nucleocrinus* Conrad, 1843. The reason assigned for retaining Roemer's name is, that Conrad (perhaps on account of the imperfection of his specimen) did not define his genus correctly. As he gave a figure of his typical species, however, *E. elegans*, that need not be confounded with other types, surely his name should stand. If we were to throw aside all the names not accompanied by correct diagnoses, great confusion would result, since many of the descriptions published by Linnæus, Müller, Link, and various other early investigators, as well as by many later ones, would apply equally well to almost any other genus of the entire family; while not unfrequently the few characters given by them were not all strictly applicable to the particular type named. Where they cited known species, however, or referred to figures, that leave no doubts in regard to the particular genera they had in view, their names have been adopted notwithstanding the defects of their diagnoses. Where an author has given us the means of knowing, beyond a reasonable doubt, what group he proposed to name, either by his diagnoses, his figures, or citations, or all taken together, his name should stand; as in the case of *Bellerophon* Montfort, which no one has ever proposed to reject, because its author described it as a chambered shell,—simply from the fact that his figure shows at a glance what genus he had in view.

Again, we cannot agree with the author of the catalogue in citing *Gilbertsocrinus* Phillips, as a synonym of *Rhodocrinus* Miller, (although he has high authority for so doing,) and placing *Goniasteroidocrinus* as a subgenus under *Rhodocrinus*. There can be little doubt in regard to *Gilbertsocrinus* and *Goniasteroidocrinus* being congeneric, though they may be subgenerically distinct.[1] That they are both distinct generically from *Rhodocrinus*, however, it is believed will be at once apparent to any one who may see a specimen of *Goniasteroidocrinus* or *Gilbertsocrinus*, with its arms and pseudo-brachial appendages unbroken. Indeed, Mr. Billings hinted at the probability that *Gilbertsocrinus* would be found to be a distinct genus, merely from inspecting Phillips's figures of imperfect specimens. The only question, in the opinion of the writer, respecting the name *Gilbertsocrinus*, is whether it may not have to give way to the older name *Ollacrinus* Cumberland, 1826. If de Koninck, Pictet and others are right in the opinion that *Ollacrinus* was founded upon one of the same types as *Gilbertsocrinus* Phillips, 1836, then Cumberland's name must take precedence, and our American species described under the names *Trematocrinus* and *Goniasteroidocrinus* will have to take the names *Ollacrinus tuberosus*, *O. fiscellus*, *O. typus*, *O. papillatus*, *O. robustus*, *O. tuberculosus*, *O. reticulatus*, &c.

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[1] See note on this subject in Proc. Phil. Acad. Nat. Sci., Aug., 1865, p. 166.

At the end of the catalogue under review the author states in a note, the *Cœliocrinus* Meek & Worthen, recently proposed for a section of *Actinocrinus*, would have to be changed, because *Cœliocrinus* had been previously applied by Dr. White to another type. This, however, is an error, since the names are not identical, *Cœlocrinus* being derived from *κοῖλος*, *hollow*, and *κρίνον*, *a lily*, in allusion to the concave base of the type; while *Cœliocrinus* is from *κοιλία*, *the belly*, and *κρίνον*, *a lily*, in allusion to the ventricose proboscis of the typical species. Hence, these names are not only different in meaning, but they are also quite as much so in sound, and to the eye, as many others now in use in various departments of natural history; such for instance as *Bulla*, *Bullia* and *Bullœa*, *Trigona* and *Trigonia*, *Astartella* and *Astartila*, as well as various others that might be mentioned in conchology alone. As a general rule, however, it is, of course, better to select names less nearly alike; but it is sometimes scarcely possible to do so if we use a descriptive name at all.

Geologists and paleontologists will certainly feel under many obligations to Dr. Shumard for the preparation of these useful lists, which will so materially facilitate their investigations; and if all cannot agree with him in every particular, it will, we think, be generally conceded that he has performed the task with skill and impartiality. M.

3. *On the deposit of Rock Salt at New Iberia, Louisiana;* by Prof. RICHARD OWEN.—Prof. Owen stated that, having heard various accounts of the rock-salt in Louisiana, he had naturally felt very anxious to examine the deposit personally. At New Iberia, La., in November last, he resigned his commission as Colonel of the 60th Indiana Regiment to accept the chair of Natural Science in the Indiana State University. Learning that the distance from New Iberia to the salt works was not great, he delayed his departure for three days for an opportunity to visit them. This was afforded him through the courtesy of Major-General Franklin; and, although the day proved very rainy, he was enabled to make a satisfactory examination of the entire locality, under the polite guidance of Mr. Henshaw.

Fifty years before this period, Mr. Marsh the father-in-law of Mr. Henshaw, had sunk a well on his plantation, "La Petite Ance," distant in a southwest direction from New Iberia about twelve miles, and only two or three miles from the Gulf of Mexico. The water from this well proving a good brine, Mr. Marsh boiled it down and made considerable quantities of salt. When, however, the demand for salt became greater, at the breaking out of the war, Mr. Marsh's son requested permission to her wells, hoping to obtain a stronger brine. After digging feet, one of the negroes employed struck a hard substance with -axe, and was desired by the owner to go on and throw out some upposed rock. On washing off the excavated mass, it proved to , hard rock-salt.

area found, at which, by probing to the depth of from 15 to 18 -salt was struck, indicates that the deposit underlies several square erhaps four to six. The materials passed through, to reach it, fly bluish clay, sand and gravel, with some lumps of micaceous ne. At the above depth, within that area, under every place at they have bored or dug down, they reached the solid rock-salt. h this solid stratum they bored twenty-six feet, and still found the osit.

tting it out for sale, it was found necessary to blast in the usual for obtaining building rock; and, even after purchasing moderate- mps, the consumer has considerable difficulty in reducing them fit for use. This compactness seems also to protect the salt from cence, and even to enable it for a long period to resist solution nmersed in water. He was assured that large lumps, packed in had been sunk in creeks and ponds for concealment, and taken ks afterwards scarcely at all diminished in bulk.

accumulation of 15 to 18 feet of clay, sand, and gravel on the had evidently been the result of comparatively recent washings e adjoining hills; and the deposit has, no doubt, been worked by rigines, as, at more than one place, on reaching the rock-salt, relics were found. He saw, at Mr. Henshaw's, a basket, obtained e surface of the rock-salt, 15 feet below the surface of the soil, f split cane; and was informed that they also found pieces of l, apparently the remnants of fires or torches. A rope of bark, hooks, stone axes and pottery were likewise obtained.

re he visited the locality, the citizens of New Iberia told him the n was volcanic, and that several similar, crater-shaped eminences along the Gulf shore. Upon close examination, however, he not nd no volcanic or other igneous rock whatever, but saw at sev- ural washes, and at cuts in the semi-circular hill, or ridge, distinct ons of successive layers of sand and gravel; the latter entirely l by attrition, and chiefly quartzose. That thrown out at the old nings was of the same character.

highest point of the ridge is 160 feet above the water in the low tide. The sea, occasionally, from the combined influence of ides and a wind blowing strongly from the south, rises in this to a considerable height, inundating the low lands and leaving rshes; which circumstance almost renders the plantation an although it is strictly peninsular. Formerly they reached it canals in the marsh by boats; but when salt became valuable, a ay, or raised road of dirt from the marsh, covered with plank, structed; and wagons came many miles to carry it off, at a cent alf per pound, delivered at the mouth of the excavation.

r an inspection of some hours, made, as remarked, rather unfavor- n account of rain, but still sufficiently in detail to be certain of the facts, and, after having obtained and closely inspected numerous specimens of the rock-salt, gravel, lumps of sandstone, and one very fine crystal, over two inches cube and nearly transparent, all of which are now in the Indiana State University, he felt assured that the whole phenomena must be referred to aqueous action.

In all probability, the semicircular deposit of sand and gravel, thrown to the height of 160 feet and conforming generally to the contour of the sea coast, resulted from the combined action of the winds and the waves of the ocean. In a similar manner, sand-ridges of nearly the same height have formed on the south shore of Lake Michigan, conforming to its coast outline; the latest and most northerly being close to the water's edge, and having formed since the settlement of the country by the white man.

As the result of similar causes, he conceived that these sea-beach ridges, on the Gulf, after being thrown up some height, permitted the high waters to flow round and into the basin-shaped depression left on the landward side, but impeded the return of the waters thus arrested.

The heat of the sun would be sufficient to evaporate the water, leaving the saline deposit; and thus, through a succession of ages, a repetition of like causes and results might readily give rise to the deposit just described. We have vegetable deposits which formed coal at various geological periods, although chiefly in the true Carboniferous era; and so we may also have saline deposits, greatest, as in Europe, during the New Red sandstone or Saliferous Period, yet taking place also during the Quaternary Epoch.

When, however, these ridges on the Gulf coast became high enough to have their materials frequently washed down by rains, the interior basin would readily fill up, and the detritus gradually cover any articles left by the aborigines. The salt and exclusion from air are sufficient to account for the preservation of the relics from decay for a long period.

The great inundation which, a few years since, destroyed so many families, who had visited Lost Island as a watering-place, was of the character above alluded to, and took place only about fifteen miles from the salt locality just described.

Whether or not the explanations here offered of the interesting phenomena exhibited at La Petite Ance is correct or not, the facts are important; and the evidences remain there to be examined at any time by those interested. The locality can be reached by railroad travel of 80 miles from New Orleans to Brashear City; thence, crossing Berwick Bay, the traveller, taking horseback or other conveyance for about 40 miles, reaches New Iberia; thence it is ten miles more to the causeway of the plantation, and two to the salt-boring; which is, as stated, on a peninsula, with Marsh Island on the south, and Vermillion Bay on the west.

The property has been sold by Mr. Marsh, and is now owned (he believed) by Mr. Avery.

It may not be irrelevant to remark, as a proof that, at no very distant period, saline inundations extended more than twenty miles inland from the present coast line of the Gulf, or at least impregnated the waters of the Bayous, that he had traced the Gnathodon (a genus of bivalves found abundantly around New Orleans, and peculiar to brackish water) along our route of march, by the Tèche, at least five miles north of Franklin, Louisiana.

He would also add, that although borings have been made to even more than 15 or 18 feet at other parts of the Gulf coast, which seemed similar in character, as yet no other considerable deposits had been found; notwithstanding that, in some places, as he understood, salt had been made from the brine springs or wells.—*Trans. Acad. Sci., St. Louis*, ii, 250.

4. *Fossil Spider from the Coal formation;* by Dr. F. Rœmer, (Jahrb. Min. of Leonhard & Geinitz, 1866, p. 136).—Dr. Rœmer has here described and figured a very perfect specimen of a spider from the Coal formation of Upper Silesia. It is called the *Protalycosa anthracophila*, a name that implies a near relation in general habit to the modern Lycosa. The body is about an inch long. Appended to this paper is a notice of a specimen of *Arthropleura armata* Jordan, from the Carboniferous beds of Zwickau, by Dr. Geinitz. The specimen is sufficient to show that the animal was a Crustacean; it is evidently part of the carapax, and probably of a Decapod.

5. *Observations on the Cretaceous strata of Texas;* by B. F. Shumard, M.D., State Geologist. 9 pp. 8vo. From the Transactions of the Acad. Sci. at St. Louis.—The paper gives an important section of the strata with descriptions and a list of fossils. The Lower Cretaceous—arenaceous and marly—is 230 feet thick, and the upper, which is the calcareous division, is 800 to 1000 feet thick in the eastern portion of the State, and of much greater thickness in the west.

6. *Report of the Chief Commissioner of Mines for the Province of Nova Scotia, for the year* 1865; by S. P. Hamilton, Provincial Secretary. 32 pp. 8vo. 1866. Halifax, N. S.—This Report treats mainly of matters of economical interest. We learn from it that the quantity of gold from the mines, on which royalty was paid, was, for the year ending 30th Sept., 1864, 18,744 oz., 5 dwt., 12 gr.; and for the following year, 24,867 oz., 5 dwt., 22 gr. Also, that the number of collieries in operation in Nova Scotia is thirty; and that the total quantity of "Round and Slack Coal" sold from the mines during the year ending Sept. 30, 1865, was 652,854 tons.

7. *Geological Survey of Nova Scotia:* Prof. How's *Report on certain minerals found by Dr. Honeymann.* 4 pp. 4to.—The minerals are ores of copper, lead and iron, barytes, limestone and "pencil stone."

8. *Sulla Geologia dell' Italia centrale:* estratto di alcune lezioni orali date nel maggio, 1864, dal Cav. Igino Cocchi, raccolte e publicate per cura di C. Puini e di A. Mariani. 100 pp. 8vo., with 2 plates. Firenze, 1864.—The author discusses with new and interesting views the nature and origin of the later formations and the features of Central Italy.

9. *Petroleum on the Alleghany River at Brady's Bend.*—Prof. J. P. Lesley has an article on the discovery of oil at Brady's Bend, and on the geology of the region, in the Proceedings of the American Philosophical Society, x, 266, 1866.

10. *Orographic Geology, or the Origin and Structure of Mountains: A Review;* by George L. Vose, Civil Engineer. 136 pp. 8vo. Boston, 1866.—The author of this work has here presented a general

review of the theories that have been brought forward to account for the disturbances, flexures or displacements, metamorphism and elevations, that have taken place in the earth's crust. These theories, and the objections to them, appear to be fairly presented, with a citation of a large number of authorities; and the work may be profitably read by all who would study this obscure department of geology. The author very judiciously does not add to the number of hypotheses, yet briefly draws some conclusions from the survey. Upon these conclusions we may remark in a future number.

11. *Dentex Münsteri, specie di Pesce i cui resti fossili, trovati nelle Argille Subapennine del Volterrano dal Dott. G. Amidei,* sono descritti ed illustrati dal Prof. C. Giuseppe Meneghini. 26 pp. 4to, with a plate. Pisa, 1864. (Nistri.)—Prof. Meneghini, besides describing a fossil fish of the genus Dentex, shows that two bones described by C. di Münster as portions of what he calls *Capitodus subtruncatus* and *C. interruptus,* belong to one and the same species, and that this species is identical with his own. He therefore gives the species the name *Dentex Münsteri.*

12. *G. F. Matthew on the Azoic and Paleozoic rocks of Southern New Brunswick,* with a map.—Mr. Matthew's valuable paper is published in the Proceedings of the Geol. Soc. London for May, 1865 (p. 422). The following are some of his conclusions: that the geological formations of Acadia include rocks of all ages, from the Huronian to the Carboniferous inclusive, with only one important hiatus in the absence of rocks of the Trenton period; that the Primordial shales are conformable to the Huronian beds; that there were disturbances at the close of the Lower Silurian as well in Acadia as in New England; others between the Upper Silurian and Lower Devonian; but far the greatest or most marked between the Devonian and the Lower Carboniferous; and perhaps a fourth between two sections of the Carboniferous system. The valuable map accompanying the paper is stated to have been prepared by Prof. L. W. Bailey and Mr. Matthew.

13. *Meteorites.*—A very important memoir on meteorites has recently been presented by Mr. Daubrée to the Academy of Sciences of Paris, and has appeared in volume lxii of the Comptes Rendus. It takes up the nature of meteorites in general, and discusses in a brief manner the following subjects: The products of the fusion of meteorites; their analogies to terrestrial rocks, and the imitation of them by treatment of these rocks; the transformation of serpentine into chrysolite, or into lherzolite, and an attempt to imitate meteorites by means of this rock; importance of magnesian rocks of the chrysolite kind both in the case of the earth and of the planetary system; new experiments illustrating the original formation of the masses from which meteorites have proceeded; application of the subject to the earth-origin of chrysolite as a universal scoria. In the fusion of serpentine from different localities (Snarum in Norway, Zœblitz in Saxony, Baldissero in Piedmont, etc.) Daubrée obtained, as the usual product, a mixture of chrysolite (silicate of iron and magnesite) and enstatite (bisilicate of magnesia). The enstatite occurs sometimes in groups of needles distributed through the crystalline chrysolite. The rock called lherzolite, from the vicinity of Lake Lherz, in France, is essentially a compound of these two minerals, chrysolite and

enstatite. From his results he concludes that serpentine has been a common source of chrysolite. The memoir merits close study.

14. *Annotated Catalogue of the principal Mineral species hitherto recognized in California, and the adjoining states and territories*, being a Report to the California State Board of Agriculture; by WM. P. BLAKE, Geologist of the Calif. State Board of Agriculture and Prof. of Min., Geol. and Mining in the College of California. 32 pp. 8vo. March, 1866. Sacramento.—Prof. Blake has done a good service to mineralogy in this catalogue of California and other west-American mineral localities. The pamphlet contains, besides a mention of the localities, notices of the associations and characters of some of the species, and a list of private and public cabinets in California. It closes with a chapter of four pages containing Notes on the Geographical distribution and Geology of the Precious Metals and Valuable Minerals of the Pacific Slope of the United States, some points in which Prof. Brewer has criticized at page 114.

15. *Die Minerale der Schweiz*, von Dr. ADOLF KENNGOTT. 460 pp., 16mo, with 78 woodcuts. Leipzig, 1866 (Wilhelm Engelmann).—Dr. Kenngott is always thorough in his mineralogical works. This Mineralogy of the Alps contains not only notices of the localities and the associations of the species, but also extended observations on peculiarities presented by many of the minerals at their several localities, with some new crystallographic determinations. The work is therefore much more than a mere topographical mineralogy. It is full of original observations.

16. *Notes on some members of the Feldspar family;* by ISAAC LEA. (From the Proc. Acad. Nat. Sci., Phil., May 1866).—Dr. Lea has here described some iridescent and other feldspars of Pennsylvania. The kinds differ somewhat in color and luster and in degree of iridescence. With the microscope he has made observations on the minute crystals of these iridescent varieties. The varieties he has observed has led him to give the name *Lennilite* to a greenish orthoclase "almost without cleavage," from Lenni, Delaware Co.; *Delawareite*, to accompanying specimens, pearly, and distinctly cleavable; and *Cassinite* to a dull bluish green, semi-transparent kind, having bright crystalline hexagonal plates within, found at Blue Hill, about two miles north of Media. Localities of sunstone and moonstone are mentioned, and some particulars respecting the microscopic crystals of different feldspars. No conclusions are arrived at in regard to the nature of the crystals.

17. *Vorlesungen über Mineralogie;* von N. VON KOKSCHAROW, Berg-Ingenieur, 1st vol., 344 pp. 4to. St. Petersburg, 1865. (A. Jacobson.) —This work is a German translation by its author from the Russian. Von Kokscharow has long been laboring with great success, and with wonderful precision through all his work, in Russian mineralogy, especially its crystallographic department. The volume just now issued is the first part of an admirable series of lessons in general mineralogy. It takes up crystallography, illustrates the subject with numerous excellent figures, many from his great work on Russian mineralogy, and throughout is both simple and thorough in its explanations. The chapter on the irregularities of crystals is particularly complete, and is made up largely of the results of his own crystallographic researches. It contains

extended tables of measurements of a considerable number of species; and is illustrated by many woodcuts, some of great beauty representing groups of Russian crystals, (chrysoberyl, etc.).

18. *On the affinities of the Bellerophontidæ;* by F. B. Meek, (Proc. Chicago Acad. Sci., i, 9).—The Bellerophontidæ are regarded by Mr. Meek as very near Emarginula, a view suggested in 1864 by de Koninck, and adopted in 1852 by d'Orbigny, and in 1855 by Pictet. His conclusions are based on a fossil described by Professor Hall under the name *Nemanotus.* Figures of this species given in McChesney's "New Palæozoic Fossils," (there called young of Bucania Chicagoënsis), and on page 344 of the Canadian Geology (1863) show that, while it has the form of Bucania, it differs in having along the middle of the dorsal side a *row of isolated oval siphonal openings.* Mr. Meek observes that it bears the same relations to *Bucania* that *Polytremaria* does to *Pleurotomaria,* and *Rimula* to *Emarginula.* In a letter to one of the editors Mr. Meek mentions that the shell is also figured in the recent paper by Prof. Winchell, on Chicago Niagara Fossils, plate 3, figs. 7*a*, *b*.

III. BOTANY AND ZOOLOGY.

1. *Boussingault's Researches on the action of Foliage.*—A full abstract of the first part of these investigations, communicated to the French Academy of Sciences, is given in the *Comptes Rendus*, vol. lx, No. 18 (May, 1865). Theodore Saussure had long ago ascertained that, while plants prosper and decompose carbonic acid gas in an atmosphere containing as much as one-twelfth or even one-eighth part of that gas, they promptly perish in unmixed carbonic acid, apparently without decomposing any of it. Boussingault made his experiments in a better form, upon leaves only, avoiding all complication of the action of the roots or other parts of the plant. His results are:

1. That leaves exposed to sunshine in pure carbonic acid do not decompose this gas at all, or only with extreme slowness.

2. But in a mixture with atmospheric air, they decompose carbonic acid rapidly. The oxygen of the atmospheric air, however, appears to play no part.

3. Leaves decompose carbonic acid in sunshine as readily when this gas is mixed with nitrogen or with hydrogen.

Although this decomposition of carbonic acid by green foliage must be a case of dissociation,—a separation of carbon from oxygen,—yet Boussingault recognizes an analogy here with an opposite phenomenon, viz., with the slow combustion of phosphorus at the ordinary temperature. Phosphorus in pure oxygen emits no light, does not sensibly undergo combustion, but does so in a mixture of oxygen with atmospheric air, or with nitrogen, hydrogen, or carbonic acid. The analogy may even be carried farther. For while a stick of phosphorus is not phosphorescent in pure oxygen at ordinary or increased pressure, it becomes so in rarified oxygen. And Boussingault equally ascertained that leaves which exerted no sensible action upon pure carbonic acid at ordinary pressure, decomposed it, with the liberation of oxygen gas, under diminished pressure. That is, rarefaction and mixture with an inert gas act alike in mechanically separating the atoms, whether of carbonic acid as n the one case, or of oxygen as in the other, so as to determine the acion either of combination or of dissociation.

In a continuation of these investigations (*Comptes Rendus*, vol. lxi, Sept. 25, 1865), Boussingault shows that carbonic oxyd, whether pure or diluted, is not decomposable by foliage, and that this inertness of green foliage upon carbonic oxyd goes to confirm the opinion maintained in his *Economie Rurale*, that leaves simultaneously decompose carbonic acid and water, $CO^2, HO = CO, H, O^2$; the O^2 being liberated, CO, H expresses the relation under which carbon is united with the elements of water in cellulose, starch, sugar, &c., i. e., in the important principles elaborated by the leaves the composition of which is represented by carbon and water. He goes on to prove that a leaf which has been decomposing carbonic acid and water all day long is capable of doing the same work the next day, if not allowed to dry, but the losing of a certain amount of water annihilates this faculty, and irremediably destroys the life of the cells of a leaf, vegetable life in this state being far less tenacious than that of some of the lower animals (*Tardigrades*, *Notipes*, &c.) which bear wonderful desiccation.

The third instalment of the investigation is given in Nos. 16 and 17 of the same volume (Oct. 16 and 23, 1865). It appears that detached leaves, kept in shade for many days, with the cut end of the petiole in water to prevent desiccation, preserve the power of decomposing carbonic acid whenever brought into sunshine. But for this they must be kept in an atmosphere containing a supply of oxygen; without this they soon die, as Boussingault thinks, from asphyxia. This oxygen in darkness is slowly transformed into carbonic acid, through an operation which is presumed to go on continually, whether in light or darkness, and to answer to respiration. Of course a healthy and active leaf decomposes far more carbonic acid in the light than it forms in darkness. In eighteen experiments, with Oleander-leaves exposed to the sun from 8 A. M. to 5 P. M. in an atmosphere rich in carbonic acid, a square meter of foliage decomposed on the average over a liter of carbonic acid per hour, while in darkness only $\frac{7}{100}$ of a liter of carbonic acid was produced per hour. In air which contains oxygen and carbonic acid leaves will go on indefinitely producing oxygen in the presence of carbonic acid, and carbonic acid in the presence of oxygen. But the latter, though relatively small in amount, seems to be necessary to the preservation of their vitality. In hydrogen, carburetted hydrogen, or nitrogen, as well as in pure carbonic acid, they soon lose their decomposing power, and die from the impossibility of respiration, i. e., are asphyxiated.

Leaves confined in a limited portion of atmospheric or other air over mercury lose the power of decomposing carbonic acid; and the experiments pretty clearly show that they lose it through the deleterious action of the vapor of mercury. It is thought remarkable that the leaf does not under these circumstances at all lose the power of transforming oxygen into carbonic acid; but that is what we should expect, for the carbonic acid so evolved (whether its evolution be called respiration or not) must be a product of decomposition of the leaf's contents or substance.

We owe to Boussingault and his assistant Lewy the idea of determining the composition of the air contained in a fertile soil, and the fact that this air in a strongly manured soil contains a very large percentage of carbonic acid. Boussingault has now devised an experiment by which the air contained in a branch of an Oleander in full vegetation was extracted. It proved to be nitrogen 88·01 per cent, oxygen 6·64, carbonic acid 5·35 per cent; being about the composition of the air from a well-manured soil. This carbonic acid carried into the leaves with the sap, and also that which they may absorb directly from the atmosphere, decomposed along with water under sunlight, must be the source of the glucose ($C^{12}H^{12}O^{12}$) which it is the principal function of foliage to produce. This glucose, in fixing or abandoning the elements of water, becomes sugar, starch, cellulose, or other hydrates of carbon, which, in whatever part of the plant accumulated or deposited, and however transformed or re-transformed, must always have originated from carbonic acid and water in the green parts of plants. In closing his present paper with some illustrations of this now familiar view, Boussingault announces that his more recent experiments will enable him to demonstrate the direct formation of saccharine matter by the green parts of vegetables exposed to the light. A. G.

2. *Revision of the North American species of Juncus;* by Dr. ENGELMANN.—No. 2 of the second volume of the *Transactions of the Academy of Science of St. Louis*, just issued, contains the following botanical papers by the indefatigable President of the Academy, viz: 1. Notes of the diagnostic characters furnished by the stone of the fruit in species of *Viburnum*, and a briefer notice of such characters in *Cornus*. 2. *Nuphar polysepalum*, a new and remarkably large-flowered species of Colorado Territory and further west, with remarks on *N. advena*, *N. luteum*, &c. This is an appendix to Dr. Parry's interesting new paper, entitled: *Notice of some additional Observations on the Physiography of the Rocky Mountains, made during the summer of* 1864. 3. And finally, this number of the Transactions is closed (on p. 458) with the 34th page of the account of our *Junci*, with which Dr. Engelmann has been occupied "since the end of last summer." The sheets now before us comprise the *generalia*, the neat systematic arrangement in a synoptical form, and the notes upon, or when needed the characters of, thirty-three of the fifty admitted North American species. Doubtless the remainder will be printed, and the memoir separately issued to botanists before this announcement is published, such is the wonderful speed with which Dr. Engelmann carries on scientific work in the midst of absorbing professional duties. For the present suffice it to say that, whereas *Juncus* has been the *dreadful* genus to North American botanists, it is likely to be dreaded no longer; that the species which at first threatened to rival *Carex* in number, are not remarkably increased by this searching revision; and, finally, that the author promises,—in case he receives the aid which we are confident his correspondents will gladly render,—to prepare and issue an "*Herbarium Juncorum Bor.-Am. Normale*, which will stand in place of expensive plates, and will, it is believed, be far preferable to them."

A. G.

3. *Lessingia germanorum.*—A friend, who knew the botanist Lessing in Berlin, informs us that Chamisso dedicated this Californian plant, not (as we have it in the last volume of this Journal, p. 263) to the botanist

and his distinguished grandfather, but to the brothers Lessing, the botanist and the painter, and so that the specific name refers not to the *Germans* but to the *brothers.* On turning to the Linnæa, however, we find all three associated in the dedication; and the specific name seems as if intended to carry a double meaning. A. G.

4. *Illustrations of the Esculent Fungi of the United States.*—We understand that our American Mycologist, Rev. Dr. M. A. Curtis, of Hillsborough, North Carolina, is preparing colored figures and descriptions of the principal eatable species of Mushrooms and other Fungi, natives of this country, with plain directions for their preparation and use. The work will be published, probably in parts, if sufficient encouragement is offered to induce a publisher to undertake it. A large amount of very nourishing and delicious food, which is wholly wasted, may be turned to good account whenever the knowledge which this work is intended to diffuse shall be made generally available. A. G.

5. *Death of Wm. Henry Harvey,* Professor of Botany in Trinity College, Dublin, and keeper of the University Herbarium.—His many friends in this country will receive with great sorrow the tidings that this distinguished Algologist as well as general botanist, and most admirable man, died of pulmonary disease, at Torquay, England, on the 15th of May, in the 56th year of his age. We may hope to present hereafter some account of his life and scientific labors. A. G.

4. *The International Horticultural Exhibition,* with a *Botanical Congress* annexed, held in London near the end of May last, appears to have been entirely successful. The managers of the Botanical Congress, which was limited to two sittings, had invited Mr. Alphonse DeCandolle to preside over its deliberations, which he did most acceptably. His address at the opening is published in the Gardener's Chronicle for May 26. In his treatment of the topics which naturally presented themselves, viz., the Advantages of Horticulture to Botany, the Advantage of Botany to Horticulture, and the beneficial Effects of the Association of Botany with Horticulture,—he rises above the commonplaces of the occasion to the consideration of important scientific questions, and to the suggestion of new modes or appliances for resolving some of them. Many of his remarks or illustrations are of such general interest that we are inclined to reprint a considerable portion of the address, if room can be found for it.

The Gardener's Chronicle for June 2 gives a list of the papers presented to the Botanical Congress, with abstracts of most of them and the details of a few. The following are those which attract our notice:

Mr. DeCandolle, the President, *On a recent very exact measurement of the diameter of the trunk of one of the gigantic Sequoias of California.* The tree was the base of "the Old Maid," the stump of which now serves as a dancing-floor; the measurement was made by Mr. De la Rue and his assistant, on a slip of paper stretched across the whole diameter of the section (26 feet, 5 inches, at 6 feet from the ground), and the rings were carefully counted and marked on the slip,—on one semi-diameter 1223, on the other 1245; the mean 1234. We imagine that this is the same tree of which we possess a similar measurement of a radius by a piece of tape, on which the centuries only are marked, making the tree about 1225 years old, as long ago recorded, we believe, in this Journal.

Prof. Caspary, of Königsberg, *On the Change in the Direction of the branches of woody plants caused by low degrees of temperature.* A full abstract of this curious paper is given; and we trust the observations will be repeated and extended in the United States and in Canada next winter. Perhaps the winter position of branches which we naturally attribute to loading with snow is partly owing to the movement which Prof. Caspary describes. The only known observation before published was made by a Mr. John Rogers in England (and recorded by Dr. Lindley in Trans. Hort. Soc. Lond., [2], ii, 230). On the remarkably cold days of January 19 and 20, 1842, when the thermometer fell to $-2°$ F. in the vicinity of London, Mr. Rogers noticed that the lower branches of a Lime-tree (*Tilia*) which overhung a part of his garden drooped so as to rest upon the ground, although there was no ice nor rime to increase their weight; and as the day advanced and grew warmer they regained their original position. Lately Von Wittich, the professor of Physiology in the University of Königsberg, noticed the same thing on a Lime-tree in his garden, and called his colleague's attention to it. Prof. Caspary found branches which were thus lowered more than three feet under a temperature of $-8\frac{1}{2}°$ F.: and last winter he proceeded to investigate the phenomenon by careful measurements, &c. We need not specify the detailed observations and experiments. The results are:

1. The branches of all trees showed a displacement in a lateral direction under severe frost, the same species moving always in the same direction, the amount of deviation increasing with the intensity of the frost. For instance, the branches of a Horse-chestnut, of a *Carpinus*, of a *Negundo*, and of a Red Buckeye moved to the left to the extent of one or two inches; those of a Lime-tree, Buckthorn, Larch, and White Pine to the right, the former as much as nearly 3½ inches, at the point measured. (The length of the branches not stated, nor why the displacement is not given in angular deviation.)

2. The Lime, Larch, White Pine, and some others exhibited a drooping displacement as soon as frost began, and they drooped the lower as the cold grew more severe, the greatest amount of the lowering in the Lime reaching to fully three feet.

3. The branches of other species of trees begin to rise as soon as frost sets in, and rise the higher the severer the frost. Examples are *Pterocarya Caucasica* and *Negundo.*

4. The branches of other species exhibit a rising motion during mild frost, but a drooping one when the cold is intense. Such are the Horse-chestnut, Buckeye, and Buckthorn. The latter two drooped at 10° F., the Horse-chestnut only at 2° 2′ F.

As the movement, whether upwards or downwards, begins only in consequence of the heat or cold penetrating the wood, it is sooner seen in thin than in thick branches. It is remarkable that the movement begun in a direction corresponding to a certain temperature, continues in that direction even after the temperature had changed to a degree that generally produces a contrary motion. Branches in the spring were found to retain a position different from that which they had in autumn under the same temperature; the same degree of cold had at different times a different effect, not always to be accounted for by any difference in the

:ion of the frost. The cause of these changes of direction Prof.
ary is unable to explain. Of course the effect of snow, water, &c.,
eliminated. The phenomenon shows that different sides of the
:hes are differently contracted longitudinally by low temperature;
vhether the shortening on the side toward which the branch turned
place only at the point of insertion or along its whole length, as is
probable, is not yet ascertained. The lateral deviation may be
:cted with the vertical by supposing the shortening of the cells
; one line of the wood, this line winding more or less in consequence
e oblique or spiral direction of the cells, as exemplified in the well
n twist of many stems, especially of *Coniferæ*. That young wood
ferently contracted by cold in different places in the direction of the
mference and radius, Prof. Caspary showed in a paper published in
lotanische Zeitung nine years ago, and he asserts that this contrac-
exceeds that of all other solid bodies, even that of zinc and iron:
he longitudinal contraction has not been examined in fresh young
. It is probably far greater than in old and dry wood, which in
is said to be 0·00000226 or 0·0000002844 per 1° F. It might ob-
ly be supposed that the change in the direction of the branches
s in direct connexion with that of the humidity of the air. The
e cold at Königsberg generally sets in with the east wind which is
dry; and so the times of frost, of great dryness, and of the greatest
ges in the direction of the branches, happen to coincide. But it is
l from a series of combined observations that, while the extremes of
erature and of change in the branches constantly coincide, those of
dity and dryness are not coincident with the extremes of change in
irection of the branches.

·. B. Clarke of London, *On the Floral Envelopes of Lauraceæ,*
e he regards as representing calyx and corolla, and the Laurels as a
of *Combretaceæ* with free perianth!

·. Hildebrand of Bonn, *On the necessity of Insect Agency in the fer-*
tion of Corydalis cava. Here, although the anthers and stigma
n contact, and the latter sure to be covered with pollen from the
er, when protected from insects no capsules set. Moreover fruit is
m formed when flowers of the same raceme are intercrossed. By
rossing of flowers of different plants only is perfect fertilization in-
l. We have been much interested this season in watching the
ual activity of so large an insect as the humble-bee in fertilizing our
'dalis aurea.

r. Howard of London, author of the *New Quinologia*, *On the Species*
nchona. He thinks that every well-defined district of the Andes
ts own prevalent and characteristic Cinchonas, and that no one spe-
prevails from end to end of the Cinchonaceous region. From the
of *C. officinalis*, raised in his own stove, he obtained as large a per-
ge of quinine as is yielded by bark of the same age in its native
try. Upon the orthography of the genus, named for the countess
chon, whether we are to write *Chinchona* or *Cinchona*, a discussion
, in which Dr. Weddell remarked that if it could be proved that
æus left out the first *h* by accident, it should be restored; but if he
erately wrote *Cinchona* for the sake of euphony, as is most probable,

it would set a bad precedent to alter it. Moreover as the *ch* in Spanish is not pronounced as in English, e. g., in *church*, no advantage would be gained by the alteration.

Prof. Karl Kock of Berlin, *Some Propositions with respect to Systematic Botany.* He thinks (as we do not) that needless multiplication of generic names would be hindered by the retention of the original describer's name suffixed to the species when transferred to another genus; that a system more prompt and effectual than that of Walper's *Annales* for collecting the scattered literature might be arranged, by a local botanical committee in every country to collect its scattered materials in the way of published names and descriptions, and a general editorship in some European capital to digest and publish them; that a botanico-horticultural congress might arrange to have the importers and raisers of novel plants get competent botanists to name them before they are dispersed; and that more botanists should become authority for single families, and work up the new plants introduced into cultivation, thus dividing the field of labor as astronomers have already divided the firmament.

Prof. Lecoq, of France, *On the Migration of Mountain Plants.* "The object of the author is to show that the mountains of Auvergne have received their alpine plants by the agency of birds and of wind, and not by a gradual migration during a supposed glacial period, the existence of which he denies altogether."

Prof. Schultz Schultzenstein, *On the presence and source of Nitrogen in turf and peat, with reference to its use as a manure for plants.* Having so far misunderstood "Ingenhauss, Saussure, Boussingault," and other sensible vegetable chemists as to suppose them to maintain that *because* "the mould of the soil is derived from the plants themselves," it "cannot serve them hereafter as food," he takes up the easy task of demonstrating the great usefulness of peat and mould in plant-growing; and then says, "it can be proved that the nitrogen of the turf really originates from animal bodies which live or have lived therein;"—"infusoria, polyps, worms, mollusks, crustaceans and insects . . . which through their bulky development produce the nitrogen in turf-pits and in turf itself." He should go on and tell us where and how these animals obtained their nitrogen!

Mr. H. Wendland of Herrenhausen, *On the culture of Palms.* The author is the successful curator of the finest European collection of living Palms. Noting that in nature most Palms have their roots in very moist soil, he was led to depart from the ordinary custom, which has small success to recommend it, and to water his Palms abundantly, even to place pans under the pots. He concludes that most Palms cannot be destroyed by too much water, but are apt to die if water is not supplied abundantly. A. G.

7. *Illustrated Catalogue of the Museum of Comparative Zoölogy at Harvard College.* No. II. North American Acalephæ. By ALEXANDER AGASSIZ.—This work, like the preceding number on Ophiuriæ, is published in a manner highly creditable both to the institution from which it emanates, and its author. The typography is excellent, and the wood-cuts, three hundred and sixty in number, are finely executed and

printed, and although most of them are in outline they serve admirably for this class of subjects. While many of the figures have appeared before in Agassiz's Contributions, vol. iii and iv, Sea-side-Studies in Natural History, and in the Proceedings of the Boston Society of Natural History, vol. ix, very many new ones appear in this work for the first time, and illustrate many species either entirely new or imperfectly described or figured before.

In addition to the descriptions of the species there is also given a great amount of information concerning their embryological development and growth and anatomical characters, which renders the work far more valuable than a mere descriptive catalogue. The synonymy is also much more complete than usual in similar works. The author has, in fact, brought together in a condensed form nearly all the information hitherto obtained concerning the species of North America, and has added very much that is new and original.

In the general remarks concerning Ctenophoræ, the various conflicting views of authors are discussed and new facts in their embryology are brought forward to prove them to constitute actually the highest order of Acalephs, and especially to show that their bilaterality is more apparent than real. The evidence adduced seems fully sufficient to establish their true position, but we cannot see that their bilaterality is destroyed thereby, or that this conflicts with their Acalephian character, since bilaterality is also a fundamental feature of both Polyps and Echinoderms. Mr. Agassiz shows that the young Ctenophore is no more bilateral than any four-rayed jelly-fish, but becomes more so by its changes during development. The author, in fact, admits their bilateral structure when he says: "Examined in the light of prophetic beings, the bilaterality of the Acalephs is but another of those wonderful links which unite in one great whole the different members of the animal kingdom" (p. 11). But on a previous page he says: "Bilaterality seems at first sight to be the plan upon which these animals are built; but an elimination of the deceptive co-efficients will show the plan of radiation underlying this apparent bilaterality. The Ctenophoræ are compared with the larvæ of Echinoderms to show the identity of plan in the two groups.

Some observations are made indicating that possibly the orders of Discophoræ and Hydroidæ, now generally admitted, may hereafter be united into one great order, which will be equivalent in value to Ctenophoræ. At the end of the volume are remarks on the geographical distribution of Acalephs, with an enumeration of the species from the different regions.

The work is one indispensable to every one interested in this class of animals, and especially so to American naturalists, since no similar work has hitherto appeared in this country relating to Acalephs. We regret only that the genera already known, but often imperfectly characterized in previous works, have not been described in this. The reason for the omission is not apparent, since in the preceding volume on Ophiurians by Mr. Lyman, the genera, both old and new, are well characterized.

A. E. V.

8. *Fossil Medusæ.*—Professor Hæckel of Jena, who in 1865 called attention to the existence of well preserved Medusæ in the lithographic slates of Eichstadt, belonging probably to the families of Æquoridæ and

Trachynemidæ, has published, in a recent number of Leonhard & Geinitz Jahrbuch, a second notice of two other species of Medusæ so well preserved that the family to which they belong can be ascertained beyond doubt. They are from the same locality, and belong to the Discophoræ, to the family of Rhizostemidæ. The restoration which Professor Hæckel has been able to make from the specimens in his possession is quite satisfactory, and the attention of geologists having been called to this subject we may expect further interesting developments in the history of Acalephæ, since it is now well known that even at the present time a kind of petrifaction of jelly-fishes when thrown upon sandy beaches readily takes place. A. A.

9. *Polymorphism among Bryozoa.*—Dr. A. F. Smith in his inaugural dissertation, published at Upsala in 1863, has shown conclusively the existence of polymorphism amoung Bryozoa. His investigations are based upon the marine species of the Scandinavian coast. He shows that there are no less than six different forms of cells, which are probably never all found on the same stock. According to his view the Avicularia are only modified cells. Stoliczka was the first to call attention to the polymorphism of Bryozoa in his studies of fossil Bryozoa. The paper by Smith is unfortunately not illustrated, and is written in a language available to but few naturalists. A. A.

10. *Anatomy and Physiology of the Vorticellidan Parasite of Hydra* (*Trichodina pediculus* Ehr.); by Prof. H. J. Clark. (16 pp. 4to. From the Mem. read before the Bost. Soc. N. H., Vol. i, Part I. Cambridge, Feb., 1866.)—Prof. Clark, through his microscopic investigations, makes this parasite of the Hydra reveal much that is important with regard to the general structure of the Vorticellidæ, while correcting many details hitherto published respecting the species. One of the points ascertained is, that the so-called vestibular lash of the Vorticellidæ, described by some microscopists, is an optical delusion.

11. *Baird's American Birds.*—Sheets 21, 22, 23, pages 320–368, of Professor Baird's work on American Birds have been issued. They treat of the Vireonidæ, including the genera *Vireosylvia* and part of *Vireo.* The pages exhibit the same complete command of the department of American ornithology so well manifested in those that have preceded them.

12. *Notes on the Embryology of Starfishes—Tornaria;* by Alexander Agassiz. 8 pp. 8vo, with a large plate. (From the Annals of the Lyc. Nat. Hist. N. Y., vol. viii, Apr. 1866.)—This paper illustrates some points of special interest connected with the relations of starfishes to other Echinoderms.

IV. ASTRONOMY.

1. *Asteroid* (86).—On the 4th of January, 1866, Dr. F. Tietjen at Berlin discovered a new asteroid of the 12th magnitude very near to asteroid (85), whose place he was employed in determining. This planet has received the name of Semele. The following elements have been furnished by Dr. Tietjen.

1866, Jan. 8·0, Berlin mean time.
M = $8^\circ\ 23'\ 14''\cdot6$
π−☊ = 300 43 14 ·3
☊ = 87 55 49 ·6 } Mean equinox 1866·0
i = 4 47 44 ·6
φ = 11 49 36 ·5
μ = 652·9848
log a = 0·490069

2. *Asteroid* (87).—On the morning of June 15, 1866, a new asteroid was discovered by Dr. C. H. F. Peters, at Hamilton College Observatory, a little brighter than stars of the twelfth magnitude. On the morning of June 21st its R. A. was $20^h\ 24^m$, and Dec. 17° 30′ S. with a slow motion toward the west.

3. *The new variable star.*—The Monthly Notices of the Royal Astronomical Society for May, 1866, contain observations of the new variable star mentioned on preceding pages of this Journal. The following results show the brightness of this star from the 15th to the 20th.

1866, May 15, at	12^h	0^m	G. M. T.	*T* Coronæ =	3·6 or 3·7
16, "	10	30	"	"	4·2
17, "	11	0	"	"	4·9
18, "	12	30	"	"	5·3
19, "	12	15	"	"	5·7
20, "	12	30	"	"	6·2

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Destruction of Scientific Museums by Fire.*—During the past two months two of the more valuable scientific collections of the country have been mostly destroyed by fire: that of the Lyceum of Natural History of New York, at the burning of the Academy of Music in May, and that of the Chicago Academy of Sciences on the 7th of June. The destruction of the former was complete. The following statement of the losses of the latter, is from a circular issued by the Academy, and signed Wm. Stimpson, Secretary. "As nearly as can now be ascertained the present condition of the collections and property of the Academy is as follows: About half the Mammals and Birds, and nearly all the skulls, etc., will be saved; the extensive collection of bird's eggs and nests was entirely destroyed; fishes and reptiles are saved; insects all destroyed with the exception of the Lepidoptera; dried Crustacea and Echinodermata destroyed; shells and fossils in great part saved. Very singularly and fortunately, the alcoholic collection, contained in about 2,000 jars, has escaped. The herbarium, with the exception of the series of the plants of the North Pacific Expedition, is saved. The Library is greatly damaged by water, but most of the books will be saved by careful drying and rebinding. The plates of the forthcoming volume of the Transactions, twenty in number, were much injured, and some of the edition may have to be reprinted. The publication of the volume, will not, however, be greatly delayed."

The lesson taught by these disasters should be heeded throughout the land: *make all buildings for scientific Museums thoroughly fire-proof.*

2. *Walker Prizes*—The founding of prizes by the late Dr. Wm. J. Walker, for memoirs presented to the Boston Society of Natural History, was mentioned in volume xl of this Journal, at page 137.

The following are the subjects for prizes, as recently announced:

Subject for 1866–7. "The fertilization of plants by the agency of insects, in reference both to cases where this agency is absolutely necessary, and where it is only accessory;" the investigations to be in preference directed to indigenous plants.

Subject of the annual prize for 1867–8. "Adduce and discuss the evidences of the co-existence of man and extinct animals, with the view of determining the limits of his antiquity."

Memoirs offered in competition for the above prizes must be forwarded on or before April first, prepaid and addressed "Boston Society of Natural History, for the Committee on the Walker Prizes, Boston, Mass."

Boston, June 1866.

3. *Rumford Medal.*—The Rumford Medal of the American Academy of Arts and Sciences was, on the 12th of June last, awarded to Mr. ALVAN CLARK of Cambridge, for his improvements in the making of lenses for the telescope.

4. *Prof. Henry A. Ward's Collections of Casts of Fossils, at Rochester, N. Y.*—Prof. Ward, in the course of his travels for the formation of his large Cabinet at Rochester, has had occasion to make casts of numerous fossils, large and small, from the skeletons of Elephants, Mastodons, and the Gaudeloupe Man to shells of Rhizopods; and he is consequently enabled to furnish copies of them to other cabinets. He is now issuing an illustrated catalogue of 150 pages or more, which gives some idea of the extent of his collections. His casts have already reached a number of scientific cabinets of the country, among them those of Yale, Amherst, Cambridge, Vassar College, Albany, etc.; and wherever they have gone they are admired for their excellence and perfection of finish. We would recommend to colleges, academies, and other institutions where science is taught in the land, to supply themselves, as far as they are able, with these casts. They enable the instructor to exhibit to students specimens of the rare fossil skeletons and other species of the rocks, many of which are seldom or never to be found in American collections. By means of them, series representing the principal types of different families (as that of Trilobites, or of Ammonites, etc.) may be made complete or nearly so. The casts are light and strong, and thus are well fitted for class purposes. They have been copied from the best specimens to be found in any collections, and are colored to correspond with the originals. They give at comparatively small expense wonderful effectiveness to a cabinet as a means of instruction. A gift of a collection of Mr. Ward's casts from any patron of learning to an academy or college would render great service to the instructor, the pupils, and the institution.

OBITUARY.

HENRY DARWIN ROGERS, one of the most widely known and distinguished of American Geologists, died on the 29th of May last, at Glasgow, in Scotland, where since 1857 he has held the chair of Regius Professor of Geology and Natural History. Prof. Rogers was born in hiladelphia in 1809, being the third of four brothers all of whom have een prominent in various departments of physical science. At the arly age of twenty-one years he became Prof. of Chemistry in Dickinon College, at Carlisle, in Pennsylvania, and not long after was appointed o the chair of Geology in the University of Pennsylvania. His duties s an active explorer in geology commenced, officially, with the Survey f the State of New Jersey, the Report and map of which he published n 1835. About a year later he was charged with the responsible duty f exploring and clearing up the geology of the great State of Pennsylania, to which difficult task he devoted many years of zealous and aithful labor, aided by a large corps of able assistants. His brother, rof. William B. Rogers, was at the same time charged with the prelimnary explorations of the State of Virginia, and the great problems of he structure of the Appalachian chain were thus at the same moment rought under the observation of two of the ablest investigators of tructural geology who have ever devoted their talents in that direction. he main features of this research, which for the first time opened up to iew the structure of half a continent, were brought before the world in masterly discussion of the whole subject in a joint memoir, communiated with an eloquence and fascination of style never surpassed, at a eeting of the American Association of Geologists and Naturalists held n Boston in the summer of 1842. This remarkable memoir, probably the most important of its class ever produced in America, is published in the volume of memoirs of the Association for that year. But these researches have their full and more perfect exhibition in the volumes of the final Report on the Geology of Pennsylvania, published in 1858, at Edinburgh, with the maps and sections executed in admirable style by A. Keith Johnston. This is the great work of Prof. Rogers's life and is an enduring monument of patient labor, originality and thoroughness of research, especially in the departments of structural and dynamic geology, taking rank with the labors of the best geologists of the time.

For some years previous to his becoming Professor at Glasgow, Prof. Rogers resided at Boston, devoting himself to his favorite studies, and to he public exposition of the departments of science which he cultivated.

His great knowledge on many subjects he was able to impart in a style equally clear and graceful, whether in public speaking or as a writer. Few teachers of science have excelled him in power of illustration of difficult subjects, or in commanding the attention of large audiences to themes not commonly discussed in public lectures. His contributions to scientific literature were numerous, and are found chiefly in the Transactions of the American Philosophical Society, the Journal of the Boston Society of Natural History, the Reports of the British Association and of its American equivalent, the Philadelphia Academy of Natural Sciences, in this Journal, and in the Edinburgh New Philosophical Journal, of which he was, for some years before his death, one of the Editors. A full list of all his published Memoirs and Reports fills an important page in American scientific history. So far as we now remember, Prof. Rogers was the only American who has been called to fill a scientific chair in a European University.

Prof. Rogers had for some years been in delicate health, but his decease was unexpected. He passed most of the last winter in Boston with his brother, Prof. William B. Rogers, returning to Scotland only a short time before his death. His amiable manners and remarkable powers as a conversationalist had won for him the same social distinction in Great Britain which he long enjoyed in America, and a numerous body of personal friends deplore his loss on both sides of the Atlantic.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Transactions of the Connecticut Academy of Arts and Sciences;* Vol. I, Part I, 248 pp. 8vo, with 3 plates. New Haven, Conn., 1866. ($2.50).—The Connecticut Academy of Arts and Sciences was organized and chartered by the State in the year 1799. In 1810 it issued the first part of Vol. I, of the "Memoirs" of the Academy, containing, among its seventeen memoirs, an article on a supposed change in the temperature of winter, by Noah Webster; on the Mineralogy of New Haven, by B. Silliman; on the quantity of rain which falls on different days of the moon, by Jeremiah Day; on an Aurora at Durham, by Rev. Elizur Goodrich; *on the Weston Meteorite,* by Profs. Silliman and Kingsley; on the theories which have been proposed to explain the origin of meteoric stones, by Prof. J. Day. Part. II of this volume appeared in 1811, Part III in 1813, and IV in 1816. Part III contains Prof. Silliman's paper on the fusion of refractory bodies by the compound blowpipe of Dr. Hare; and Prof. J. Day's on the comet of 1811.

Since 1816, papers read before the Academy have, to a considerable extent, found their way to the public through the American Journal of Science, the first number of which was issued in August, 1818. The Academy has now commenced a second series of publications under the title of Transactions. The volume just issued contains four papers: 1, The register of the Aurora Borealis made at New Haven by E. C. Herrick, between March 1837 and May 1854, occupying 130 pages, with extracts from a register by Francis Bradley; 2, Notices of Auroras, extracted from the Meteorological Journal of Rev. Ezra Stiles, S.T.D., President of Yale College, made between Nov. 1763 and Nov. 1794, together with other miscellaneous notices of later date collected by Prof. E. Loomis; 3, on Bekker's Digammated text of Homer, by Prof. James Hadley; 4, on the mean temperature and on the fluctuations of temperature at New Haven, as deduced from 86 years of Observations, by Professors Elias Loomis and H. A. Newton, illustrated by three plates, one showing the mean daily curve of temperature for each month, and the other two giving chrono-isothermal lines between the mean and the highest and lowest temperature.

The Academy solicits exchange of publications from other academies, and announces on the cover of the volume that packages may be addressed to the Librarian of the Academy at New Haven.

2. *The American Annual Cyclopædia and Register of important events of the year* 1865. vol. v, large 8vo. New York, D. Appleton & Co. 1866. pp. 850.—It is highly creditable alike to the Editors and the great publishing house of Appleton that the interesting and important events of the year 1865 should so early appear in a systematic form, embracing political, civil, military and social affairs, public docu-

biography, statistics, commerce, finance, literature, science, agri- and mechanical industry.

chief articles on scientific subjects are treated with a good degree ess of detail, and for the most part with excellent judgment. the topics of interest to scientific men we notice the articles on my, observatories, and instruments, auroras, meteors, and meteor- luding Prof. Newton's researches, and the height of the atmos- hemistry, chemical arts and the new nomenclature and notation mistry, geographical explorations and discoveries, magnesium, n, and the metals generally; disease of swine, the telegraph, &c. of American and foreign obituaries is also quite full and able. important advantage to the reading public to have access to so te a summary of scientific progress as is offered in these pages. ant omissions might be named, but they are mostly of topics in a previous or a following year have been or may be discussed

subjects of paramount importance in public affairs for the year such as army operations, naval affairs, and the proceedings of ss, justly receive in this volume a larger portion of space than ers; larger, probably than they may ever do again.

hambers's Encyclopedia: a Dictionary of Universal Knowledge People, on the basis of the latest edition of the German Conver- -Lexicon. Illustrated by wood-engravings and maps. Philadel- (J. B. Lippincott & Co.)—Parts 102–106 of Lippincott's reprint mbers's Encyclopedia have recently been issued. No. 106 closes viii, and ends with the word Sound. This number contains, its interesting notices, an account of the recently discovered fossil Solenhofen, with a woodcut. The scientific articles, with their tions, are an important part of this Encyclopedia.

nnals of the Dudley Observatory, Vol. I. lxvii and 126 pp. 8vo, n appendix of [126] pages.—This volume from the Dudley Ob- ry contains a description of the observatory and its instruments. s with a landscape view in lithography, and gives full details of ucture within, illustrating its interior arrangements by woodcuts. with the descriptions of the instruments there are: a large copper- ngraving of the fine equatorial on a scale of one-twelfth; others Olcott meridian circle and its parts; one of the transit instrument y Pistor & Martius in Berlin in 1860; others of the comet seeker; ronographic apparatus; declinometer. Various topics are dis- which are of importance to the practical astronomer. The Ap- contains, in addition to other matters, observations of the planet made at the observatory in 1862; of the planet Neptune, made in and of asteroids.

lmsted's Astronomy: an Introduction to Astronomy, designed as -book for the use of Students in College; by DENISON OLMSTED, late Prof. Nat. Phil. and Astron. in Yale College. Third edition. d by E. S. SNELL, LL.D., Prof. Nat. Phil. in Amherst College. p. 8vo, with five plates and numerous woodcuts. New York. (Collins & Brother.)—This popular text-book has undergone a vision by Prof. Snell, much improving it. The changes consist in sations, the omission of some historical paragraphs, a few alterations in the arrangement, and the introduction largely of new engravings, some as substitutes for old cuts, and others in illustration of points not before discussed. The volume closes with some useful tables. The work is clear, simple, and sufficiently full for ordinary class instruction.

Transactions of the American Institute of the City of New York for 1864, '65. 732 pp. 8vo. Albany, 1865.

The Comstock Lode, its character and the probable mode of its continuance in depth, by F. B. Richthoven, Dr. Phil. 84 pp. 8vo. San Francisco, 1866.

History of the Chicago Artesian well: by Geo. A. Shufeldt, Jr. 50 pp. 8vo. Chicago, 1866.

Proceedings Acad. Nat. Sci. Philad., No. 1. Jan., Feb., March, 1866.—Page 10, A study of the Icteridæ; *J. Cassin.*—p. 25, Critical Review of the Procellariadæ, Part iii, embracing the Fulmareæ; *E. Coues.*—p. 33, Twelve new species of Unionidæ; *I. Lea.*—p. 35, Fasti Ornithologiæ; *J. Cassin.*—p. 39, List of the Birds of Fort Whipple, Arizona, with brief critical and field notes; *E. Coues.*

Proceedings Boston Soc. Nat. Hist., Vol. X.—Page 180, On the genus Belemnocrinus; *C. A. White.*—p. 102, Habits of the Halibut; *N. E. Atwood.*—p. 187, On some Odonata from the Isle of Pines; *S. H. Scudder.*—p. 200, On the spider *Nephila plumipes* and its silk as an economical product; *B. G. Wilder.*—p. 211, On some Odonata from the White Mts., N. H.; *S. H. Scudder.*—p. 223, On the Trichodina pediculus *Ehr.; H J. Clark.*—p. 224, Notes on a tour in California and Nevada; *C. T. Jackson.*—p. 229, Notes on Hawaian volcano; *H. Mann.*—p. 231, On the vestibular bristle of Vorticellidæ; *H. J. Clark.*—p. 236, Earthquake at San Francisco; *W. P. Blake.*—p. 237, Elevation of Continental masses; *N. S. Shaler.*—p. 241, On the Pleistocene Glacial Climate of Europe; *H. D. Rogers.*—p. 248, List of Birds from Porto Rico; *H. Bryant.*—p. 257, New preservative solution for specimens; *A. E. Verrill.*—p. 259, Note on Geog. Distribution of N. A. birds; *A. E. Verrill.*—p. 262, Notes on California; *C. T. Jackson.*—p. 264, List of Vertebrates of Okak, Labrador, observed by Rev. S. Weiz; *A. S. Packard, Jr.*—p. 279, On the development and position of the Hymenoptera, with notes on the Morphology of Insects; *A. S. Packard.*

Proceedings Chicago Acad. Sci., Vol. I.—Page 9, Note on the affinities of the Bellerophontidæ; *F. B. Meek* —p. 11, Description of Paleozoic fossils from the Silurian, Devonian and Carboniferous of Illinois and other Western States; *Meek & Worthen.*—p. 33, On a new species of the genus Macrorhinus; *T. Gill.*—p. 46, Descriptions of new Macrurous Crustacea from the coasts of N. America; *W. Stimpson.*

The officers of the Academy are Edmund Andrews, M.D., President, Daniel Thompson and Benjamin F. Culver, Vice Presidents, Wm. Stimpson, M.D., Secretary, G. H. Frost, Librarian.

Proceedings of the Essex Institute, Vol. IV, No. 8. Oct., Nov., Dec., 1865. Issued June 1, 1866. Salem, Mass.—Page 197, Observations on Polyzoa, suborder Phylactolæmata; *A. Hyatt.* A very valuable paper, illustrated by 14 plates of unexcelled beauty.

Transactions of the Acad. Sci. of St. Louis. Vol. II, No. 2, 1866.—Page 222, also 226, 246, 249, 264, 266, 297, 298, 419, On climate of St. Louis, etc.; *Engelmann.*—p. 223, Ancient graves in Pike Co., Mo.; *Broadhead.*—p. 224, Gestation of Opossum; *Engelmann.*—p. 226, On P. E. Chase's intellectual symbolism; *Holmes.* p. 250, Rock salt deposit in Louisiana; *Owen.*—p. 260, A new Icterus; *Shimer.*—p. 263, Oil springs in Missouri; *Shumard.*—p. 272, Physiography of the Rocky Mts.; *Parry.*—p. 282, On Nuphar polysepalum; *Engelmann.*—p. 285, Altitude of Long Peak; *Engelmann.*—pp. 287 and 414, On atmospheric electricity; *Wislizenus.* —p. 299, Thoughts on Matter and Force; *Wislizenus.*—p. 311, Coal-measures in Missouri; *Broadhead.*—p. 334, Catalogue of the N. A. Paleozoic Echinodermata; *Shumard.*—p. 408, New var. of Spirifer; *Swallow.*—p. 410, New Bryozoa; *Prout.* —p. 417, Observations on Ozone; *Bandelier.*—p. 418, Fossil horse in Kansas; *Swallow.*—p. 424, Revision of the N. A. species of Juncus; *Engelmann.*

THE

AMERICAN

RNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

XI.—*Results of Magnetical Observations made at Eastport, e, between* 1860 *and* 1864, *for the United States Coast Sur-* communicated by A. D. BACHE, Supdt., under author- rom the Treasury Department.

SE observations were made in connexion with the general of magnetical determinations on the coast, and with the object of ascertaining the law of the secular change in ternmost coast region of the United States.

he Eastport station, selected for that purpose, the plan of equired the observation of the magnetic declination, dip rizontal intensity, during three days near the middle of onth. It is worthy of remark, as a result of these obser- , continued for over four years, that the most important tic features of a locality may be developed by a system rvations requiring comparatively but a small sacrifice of uch as is in the power of many persons to make, who gaged in other pursuits.

instruments used were a theodolite magnetometer and a cle, both constructed by Mr. Wm. Wurdemann, according gns by Mr. J. E. Hilgard.

principal novelty in the theodolite magnetometer, besides details affording greater convenience in making the nents than heretofore had, consists in having the colli- magnets so light as to be readily supported by a single er, the torsion resistance of which is extremely small, and tible of a stable adjustment. The magnets are 3½ inches long, $\frac{5}{16}$ inch external and $\frac{4}{16}$ internal diameter; value of a scale division 15″. The dip-circle has a diameter of $5\frac{1}{2}$ inches, and reads to 30″ by means of two verniers; the needles are $9\frac{1}{2}$ inches long, and the pointings are made with microscopes attached to the vernier-arms, on small holes pierced through the needles. Those used during the first half of the series have axles and pivots of the ordinary construction, but from October 1862 two needles were used having their axles so fitted in arbors, as to admit of being turned about their centers, by which means they may be brought to rest on different parts of the pivots. The observations were made on each day in three different positions of the pivots, and the errors arising from faults in their figure appear to be very nearly eliminated.

The position of the magnetic observatory is on the parade ground of Fort Sullivan near Eastport, in lat. 44° 54′·4, long. 66° 58′·9, west of Greenwich.

The observations were made successively by Messrs. G. B. Vose, S. Walker, E. Goodfellow, A. T. Mosman and H. W. Richardson, all attached to the Coast Survey.

The discussion herewith presented of the observations, has been made by Assistant C. A. Schott. The results are stated under the several heads of declination, dip and horizontal intensity.

1. *Declination.*—The zero of the collimator magnet, or the position of the magnetic axis on its scale, was determined by inversions each month, and its readings on the circle referred to a distant mark of known azimuth. On four days about the middle of each month the declination readings were recorded every half hour, between the morning minimum and the afternoon maximum, generally between the hours of 6 A. M. and 2 P. M. Each monthly declination result is therefore the mean from observations made on four days.

To obtain the mean declination of the day or that value which would result from 24 hourly observations, a small correction is applied, derived from the discussion of the Girard College series, which shows that the mean of the least and greatest declination of the day gives the *west* declination too large, as does also the mean of the half hourly readings between those extremes, the amount of excess being the same. We have accordingly the following corrections to our means *expressed in parts of the diurnal range:*

In January,	$-\frac{1}{10}$	In July,	-0
" February,	$-\frac{1}{15}$	" August,	-0
" March,	$-\frac{1}{20}$	" September,	$-\frac{1}{25}$
" April,	$-\frac{1}{20}$	" October,	$-\frac{1}{10}$
" May,	-0	" November,	$-\frac{1}{10}$
" June,	-0	" December,	$-\frac{1}{8}$

)iurnal range of the declination.—The difference between the ximum and minimum value of the declination set down in table for each month, is the mean of four days of observa-ıs.

	1860.	1861.	1862.	1863.	1864.	Means of 5 years.	Observed annual inequality.	Cor. for 11 years. inequality	Corrected annual inequality.
.,	15′·2	8′·6	12′·1	10′·6	11′·4	11′·6	−2′·1	−0′·4	−2′·5
.,	12·6	11·7	11·0	8·0	9·0	10·5	−3·2	−0·3	−3·5
rch,	16·4	18·1	13·1	11·8	13·0	14·5	+0·8	−0·2	+0·6
il,	16·0	13·6	18·4	16·6	11·6	15·2	+1·5	−0·1	+1·4
r,	18·7	8·8	14·2	15·6	15·9	14·6	+0·9	0·0	+0·9
e,	18·1	12·3	17·7	15·5	13·2	15·4	+1·7	0·0	+1·7
r,	13·0	16·3	17·8	14·6	13·6	15·1	+1·4	0·0	+1·4
.,	21·5	18·7	19·8	14·5	...	18·3	+4·6	0·0	+4·6
t.,	19·5	18·8	17·4	15·5	...	17·5	+3·8	+0·1	+3·9
,	18·7	10·4	14·1	14·2	...	14·0	+0·3	+0·2	+0·5
.,	13·7	9·2	8·9	8·9	...	9·9	−3·8	+0·3	−3·5
.,	7·8	9·0	8·0	6·4	...	7·5	−6·2	+0·4	−5·8
ıual ıns,	15·9	13·0	14·4	12·7	...	13·7			

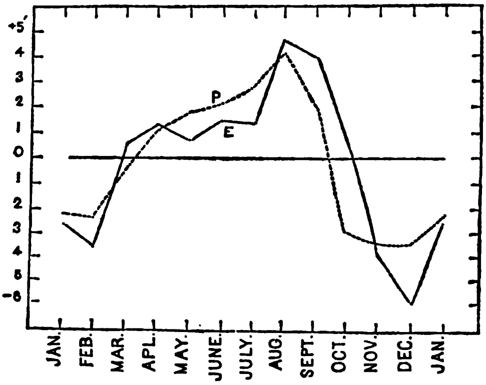
'he average difference between the 7 values of 1864 and the ın of the 4 years preceding is −1′·6. Applying this to the 4 rs means for the remaining five months, the interpolated means 1864 become, for Aug. 17′·0, for Sept. 16′·2, for Oct. 12′·8, Nov. 8′·6, and for Dec. 6′·2. The column headed "means of ears" is completed with the aid of these values. The inter-ıted mean for 1864 is 12′·4. The average annual change - 0′·9.

'he 11 year inequality in the range of the diurnal movement ears quite plainly in the annual means. The year 1860 was of maximum and 1866 of minimum, according to the ob-rations of the solar spots; in 1865 the average range will, refore, be a little above 12′, and in 1866 a little below this ue, giving a range of variability due to the 11 years inequal-of nearly 4′. The corresponding quantity at Philadelphia early 2′ from observations between 1840 and 1845.

)ur series of observations extends over less than one half of eleven year period, a correction has therefore been applied ɔbtain the annual inequality in the diurnal range free from eleven year period as shown in last column of above table.

The annual inequality of the diurnal range at Eastport and iladelphia compare as shown by the annexed diagram, the l line being for Eastport, the broken line for Philadelphia. e Toronto curve also agrees well with these curves.

ANNUAL INEQUALITY IN THE DIURNAL RANGE.



The diurnal range reaches a maximum in August and a minimum in December. There is reason to suppose that the curve is a compound one, consisting of two waves, changing its character according to changes in epochs and amount of these component systems.

Epochs of greatest diurnal deflection.—The average epochs of the morning east elongation and the afternoon west elongation are given in the following table:

	East Elongation.		West Elongation.			East Elongation.		West Elongation.	
	H.	M.	H.	M.		H.	M.	H.	M.
Jan.,	8	30	1	20	July,	7	20	1	00
Feb.,	8	40	1	50	Aug.,	7	10	0	20
Mar.,	8	20	1	10	Sept.,	7	30	0	50
Apr.,	7	50	1	10	Oct.,	7	50	1	10
May,	7	10	0	20	Nov.,	8	00	1	00
June,	6	50	0	40	Dec.,	9	00	1	00

For the summer half year from April to September included, the morning east elongation occurs at $7^h\ 20^m$, and for the winter half year from October to March included, the east elongation occurs at $8^h\ 20^m$; at Philadelphia these epochs were $7^h\ 33^m$ and $8^h\ 24^m$ respectively. For the summer half year the afternoon west elongation occurs at $0^h\ 40^m$, and for the winter half year at $1^h\ 20^m$. At Philadelphia these epochs were $1^h\ 8^m$ and $1^h\ 25^m$ respectively. On the average for the year, the turning epochs are $7^h\ 50^m$ A. M., and $1^h\ 0^m$ P. M.

Mean monthly values of the declination, observed at Eastport between August 1860 and July 1864.

These values were obtained as follows: Let $D_{,}$ = mean of daily minimum and maximum declination, $D_{,,}$= mean of all half hourly declinations, between these extremes and including them, then $D = \frac{D_{,}+D_{,,}}{2} + C$, where C = correction to refer the

ination to its average value of the day. The minutes given
he table are to be added to 17°.

	1860, 1861.	1861, 1862.	1862, 1863.	1863, 1864.	Means.
Aug.,	58′·1	59′·5	60′·1	64′·2	60′·5
Sept.,	56·9	61·1	61·1	63·6	60·7
Oct.,	57·2	60·1	60·8	63·7	60·5
Nov.,	59·8	61·7	63·5	64·3	62·3
Dec.,	58·5	59·2	62·3	62·7	60·6
Jan.,	56·5	61·0	62·1	62·8	60·6
Feb.,	58·1	60·6	60·5	62·5	60·4
March,	58·1	58·4	61·7	63·4	60·4
April,	58·7	61·2	62·0	63·0	61·2
May,	58·0	60·1	61·1	61·6	60·2
June,	61·0	59·3	60·7	61·2	60·5
July,	58·1	59·2	60·7	62·0	60·0
Means,	58·2	60·1	61·4	62·9	60·65

'he average value for the period is 18° 00′·65.
nnual effect of the secular change.—We deduce the annual
ct of the secular change directly from the preceding table.

Annual increase of declination between 1861 and 1862, 1′·9
" " " 1862 " 1863, 1·3
" " " 1863 " 1864, 1·5

Average annual increase of west declination, 1·6

ich, considering the locality, appears a remarkably small
ue. According to our previous information we might have
ected an annual increase of about 4′. Either the above
ll result indicates a local deviation from the general law, or
at this most *easterly* station we are approaching the period
stationary condition which, from previous researches, may
xpected to take place before the close of the present century.
n July 1865 the declination was again observed in order to
ain a confirmation of its small annual increase; from four
's of observation 18° 04′·7 was found, and since the annual
n is found by adding 1′·4 (vide previous years) the declina-
for 1865 becomes 18° 06′·1, and the annual increase appar-
ly equals 2′·4.
'he annual mean declination corrected for imperfect number
observations in 1860 and 1864, is as follows:

In 1860,	17° 57′·1 W.	In 1863,	18° 02′·3 W.
" 1861,	17° 59′·2 "	" 1864,	18° 03′·7 "
" 1862,	18° 00′·6 "	" 1865,	18° 06′·1 "

nnual inequality of the declination.—The difficulty of estab-
ing this inequality experimentally is well known, and long
tinued and frequent observations have failed to furnish a
isfactory general elucidation of this subject, in reference to
ich the Coast Survey Report for 1860, pp. 311–12, may be
sulted.

The values for Eastport have been derived as follows:

	17°+	Correct'ns for secular change.	Corrected declination.	Annual inequality.
Aug.,	60·5	+0·7	61·2	+0·55
Sept.,	60·7	+0·6	61·3	+0·65
Oct.,	60·5	+0·5	61·0	+0·35
Nov.,	62·3	+0·3	62·6	+1·95
Dec.,	60·6	+0·2	60·8	+0·15
Jan.,	60·6	+0·1	60·7	+0·05
Feb.,	60·4	−0·1	60·3	−0·35
March,	60·4	−0·2	60·2	−0·45
April,	61·2	−0·3	60·9	+0·25
May,	60·2	−0·5	59·7	−0·95
June,	60·5	−0·6	59·9	−0·75
July,	60·0	−0·7	59·3	−1·35

The following comparative table contains the annual inequality for Eastport, Philadelphia and Toronto, the latter for three different epochs, the last two of which are derived from Mr. Kingston's paper on "Monthly absolute values of the magnetic elements at Toronto, from 1856 to 1864 inclusive:"

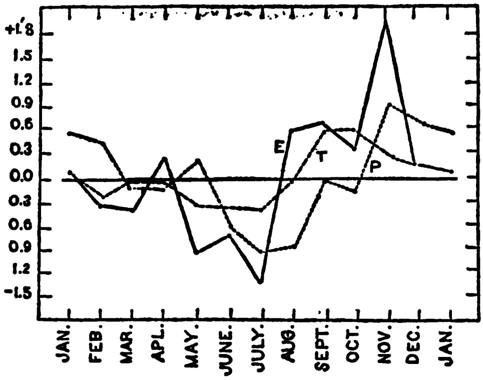
Annual inequality of the Magnetic Declination. + indicates west deflection, − east deflection from Normal declination.

	Eastport, 4 years, 1860-64.	Phila. 5 years, 1840-45.	Toronto. 7 years. 1845-51.	Toronto. 4 years, 1856-59.	Toronto. 5 years, 1860-64.	Toronto. 16 years.
Jan.,	+0·05	+0·5	+0·1	−0·2	+0·10	+0·03
Feb.,	−0·35	+0·4	−0·5	+0·2	−0·35	−0·28
March,	−0·45	−0·1	−0·2	+0·5	−0·14	0·00
April,	+0·25	−0·1	0·0	+0·1	−0·31	−0·07
May,	−0·95	+0·2	−0·1	−0·4	−0·67	−0·35
June,	−0·75	−0·6	−0·5	−0·7	+0·03	−0·38
July,	−1·35	−1·0	−0·8	−0·5	+0·30	−0·38
Aug.,	+0·55	−0·9	−0·2	−0·1	+0·19	−0·06
Sept.,	+0·65	0·0	+0·7	+0·7	+0·13	+0·52
Oct.,	+0·35	−0·2	+1·0	0·0	+0·40	+0·56
Nov.,	+1·95	+0·9	+0·3	+0·1	+0·41	+0·29
Dec.,	+0·15	+0·7	+0·3	+0·1	−0·07	+0·14

The general agreement of the Eastport, Philadelphia, and the approximate resemblance of the Toronto (16 year) curves is shown on the annexed diagram; the annual range keeps probably within 2′, and it is not clear whether the annual inequality is subject to a variation of a comparatively short period, a question which remains to be cleared up by future observations. The effect of the annual inequality is to diminish the west declination in July and to increase it in November, these being the months when it reaches its greatest amount.

2. *Dip.*—The series of observations and results extends from January 1860, to July 1864, during which period the instruments and observers were changed several times. A partial discussion of the observations of 1860–61–62 showed an annual

iminution of the dip of 2′·2 in the first year, and of 3′·0 in the econd year. These evidences of a *decreasing* secular change



appeared at that time anomalous, but it will be seen that they are borne out by subsequent observations, and likewise in other places. The observations of the dip at Washington in 1860 first *indicated* a change of sign in the secular effect, which fact is now fully established by later observations.

The values given in the following table for each month are the means of observations made on three, and sometimes on four days with two needles; the polarity of the needles was reversed during each set:

Monthly means of magnetic dip.

	1860.	1861.	1862.	1863.	1864.
	° ′	° ′	° ′	° ′	° ′
Jan.,	75 51·6	75 52 9	75 50 2	75 47·0	75 46·1
Feb.,	52·7	53·4	49·5	45·6	46·7
March,	54·5	52·7	49 0	47 4	46·7
April,	54·4	52·1	49·3	49 0	44·6
May,	53·5	49·5	48·1	47·9	46·0
June,	51·8	49·8	48·0	48·6	45·6
July,	52·9	49 9	48·4	49 1	44·9
Aug.,	54·7	50 4	48 2	50·6	
Sept.,	53·5	51·2	48·3	49 8	
Oct.,	53·3	50·9	49·2	49·5	
Nov.,	51·9	49·9	47·4	48·3	
Dec.,	52 3	49·3	46 9	46·8	
Means,	75 53·1	75 51 0	75 48·5	75 48·3	

In July 1865 the dip was found from observations on four days, 75° 44′·7, which, when reduced to the mean of the year, gives 75° 44′·8 for 1865.

Annual effect of the secular change.

1861—1860,	−2·1
1862—1861,	−2·5
1863—1862,	−0·2
1864—1863,	−2·0
Mean.	−1·7

On the average, therefore, the annual diminution is very nearly 1′¾.

Annual inequality of the dip.—If we take the monthly means for the four years 1860-1863, correct them for secular change, the monthly effect of which is −0′·14, and then take the differences of each value from the general mean, we shall have the annual inequality, as given in the following table, in which for comparison is likewise given the annual inequality for Toronto, derived by the same treatment from the results given by Mr. Kingston. The annual effect of the secular change for Toronto between 1860 and 1864 is −0′′·90, about half that of Eastport.

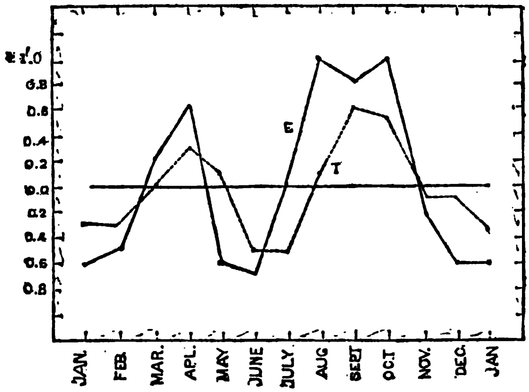
A + sign indicates greater dip, a − sign less dip than the normal value.

	Observed means.	Corrections for secular change.	Corrected dip.	Annual inequality.	
				Eastport.	Toronto.
	° ′	′	° ′	′	′
Jan.,	75 50·4	−0·77	75 49·6	−0·6	−0·3
Feb.,	50·3	−0·63	49 7	−0·5	−0·3
March,	50·9	−0·49	50·4	+0·2	+0·1
April,	51·2	−0·35	50·8	+0·6	+0·3
May,	49·8	−0·21	49·6	−0 6	+0·1
June,	49·6	−0·07	49·5	−0·7	−0·5
July,	50·1	+0·07	50·2	0·0	−0·5
Aug.,	51·0	+0·21	51·2	+1·0	+0·1
Sept.,	50·7	+0·35	51·0	+0·8	+0·6
Oct.,	50·7	+0·49	51·2	+1·0	+0 5
Nov.,	49·4	+0·63	50·0	−0·2	−0·1
Dec.,	48·8	+0·77	49·6	−0·6	−0·1
Mean,	75 50·2				

The law for the two stations is evidently the same as shown by the annexed diagram. The range of the inequality at Toronto is less than at Eastport, where it hardly reaches +1′ and −1′. The dip is greater about the equinoxes and less about the solstices, also greatest at the autumnal equinox and least at the winter solstice.

3. *Horizontal intensity.*—The observations for horizontal intensity were made by vibrations and deflections, with the theodolite magnetometer, on four days near the middle of each month. The moment of inertia and temperature coefficient of the magnet employed were ascertained by numerous experiments. In order to convey an idea of the accuracy of the

servations, the values of the magnetic moment of the magnet e subjoined as resulting from the monthly determinations dur-



ng the last two years when its magnetic condition had become nearly constant.

Magnetic moment of magnet A *at* 62° *Fahr.*

1862.	July,	0·4017	1863,	0·4004
	Aug.,	·4015		·3999
	Sept.,	·4013		·4003
	Oct.,	·4016		·4003
	Nov.,	·4012		·4003
	Dec.,	·4009		·4000
1863.	Jan.,	·4007	1864,	·4002
	Feb.,	·4006		·4001
	Mar.,	·4007		·4003
	Apr.,	·4008		·4003
	May,	·4008		·4007
	June,	·4006		·4001

Table of observed values of the horizontal force.

	1860.	1861.	1862.	1863.	1864.	Mean 1860-64, 5 years.
Jan.,	3·298	3·306	3·297	3·304	3·308	3·303
Feb.,	3·299	3·311	3·300	3·304	3·308	3·304
March,	3·300	3·308	3·302	3·307	3·311	3·306
April,	3·311	3·307	3·302	3·314	3·313	3·309
May,	3·309	3·315	3·307	3·318	3·315	3·313
June,	3·318	3·316	3·309	3·314	3·320	3·314
July,	3·315	3·316	3·305	3·313	3·320	3·314
Aug.,	3·306	3·305	3·306	3·311	....	3·308
Sept.,	3·307	3·308	3·304	3·308	....	3·308
Oct.,	3·307	3·297	3·303	3·306	....	3·304
Nov.,	3·308	3·297	3·304	3·308	....	3·305
Dec.,	3·309	3·297	3·302	3·308	....	3·305
Mean,	3·307	3·307	3·303	3·310		3·308

In computing the last column the wanting values for 1864 have been supplied by interpolation. In July 1865, from observations on 4 days the horizontal force was found 3·319; referring this to the annual mean we subtract 0·006 and obtain for 1865 the value 3·313.

Secular change of the horizontal force.—Examining the annual means we notice at first a diminution of the force till the beginning of the year 1862; the force after this date shows an annual increase of 0·0012 parts of the force. In a previous discussion (Coast Survey Report 1861, Appendix No. 22) the horizontal force along the Atlantic coast was found to diminish annually 0·0011 parts of the force. This diminution, according to the Eastport observations, has now ceased, and changed to an increase. This reversal of the change corresponds to the observed diminution of the dip, and is supported by the Toronto observations, where it took place, according to G. T. Kingston, Director of the Observatory, in 1860, which is the year of minimum force. At Toronto the present annual increase amounts to 0 0010 parts of the force.

We have further from the Eastport observations the following table of the total force, F=H sec I.

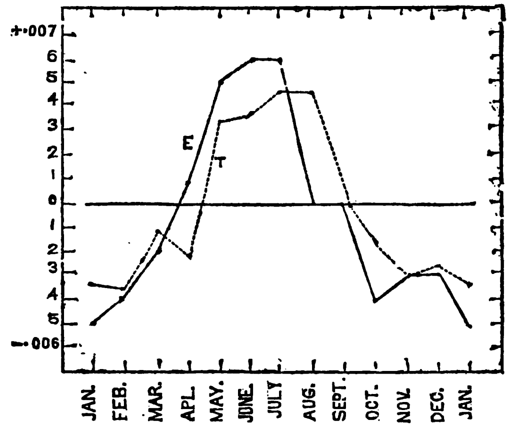
	I.	H.	F.
1860,	75° 53′·1	3·307	13·56
1861,	75° 51′·0	3·307	13·53
1862,	75° 48′·5	3·303	13·47
1863,	75° 48′·3	3·310	13·50
1864,	75° 46′·3	3·313	13·48

The total force appears to be decreasing in conformity with the diminution of the dip; at Toronto it has likewise decreased between 1845 and 1864. From observations at Key West, Florida, it appears that both the horizontal and total forces were diminishing between 1860–1864.

Annual inequality of the horizontal force.—The annual variation can be found directly by subtracting the annual mean from each monthly result, since the minimum year is nearly midway between the extreme epochs. At Toronto, likewise, the minimum year is midway between the extreme years; the series there extends over nine years, between 1856 and 1864.

ual inequality of the horizontal force, at Eastport 1860–1864 (4½ *years*); *at Toronto* 1856–1864 (9 *years*).

	H at Eastport.	Annual inequality, Eastport.	H at Toronto.	Annual inequality, Toronto.
Jan.,	3·303	−·005	3·4850	−·0034
Feb.,	·304	−·004	·4847	−·0037
March,	·306	−·002	·4873	−·0011
April,	·309	+·001	·4863	−·0021
May,	·313	+·005	·4918	+·0034
June,	·314	+·006	·4921	+·0037
July,	·314	+·006	·4931	+·0047
Aug.,	·308	·000	·4930	+·0046
Sept.,	·308	·000	·4889	+·0005
Oct.,	·304	−·004	·4868	−·0016
Nov.,	·305	−·003	·4855	−·0029
Dec.,	·305	−·003	·4858	−·0026
Means,	3·308		3·4884	



ie agreement in the annual inequality at the two places is ose as can possibly be expected. For stricter comparison it d be necessary to convert the tabular numbers of the uality into parts of the respective forces.

. XXII.—*On the Age of the Coal Formation of China;* by J. S. Newberry: addressed to Raphael Pumpelly, Esq.

IE fossil plants you were kind enough to submit to me for ination, though few in number and somewhat fragmentary, proved to be of very special interest since they supply the ssary data for determining approximately the age of the a from which they were taken, and rather unexpectedly e a large part of the great coal-fields of China to be of zoic age.

This conclusion is based on the entire absence of Carboniferous plants from the collection; and the presence of well marked Cycads—species of *Podozamites* and *Pterozamites*—closely allied to, if not identical with, some heretofore found in Europe and America.

I give below such descriptions of the several species contained in the collection as could be framed from the somewhat meager material submitted to me. Future observations made upon a larger number of more perfect specimens will be necessary before questions of specific identity or difference can be definitively settled, but it is scarcely probable that any facts or specimens hereafter to be obtained will require a modification of the view, that the coal basins which you visited are all Mesozoic, and not Carboniferous.

We have, of course, no right to assume from the interesting facts your explorations have brought to light, that no Carboniferous coal exists in China, for it may very well happen that, as in our own country, coal seams of economical value, but of different ages, will be found there at points not greatly removed from each other; but geologists will not fail to be deeply interested in the fact that so large portions of the coal basins of China, including beds of both anthracite and bituminous coal, worked for hundreds of years—probably the oldest coal mines in the world—are wholly excluded from the Carboniferous formation. So large is this coal-bearing area, indeed, that when joined to the Triassic, Cretaceous and Tertiary coals of North America, they quite overshadow the Carboniferous coals of Europe and the Mississippi valley, and suggest the question whether the name given to the formation, which includes the most important European coal strata has not been somewhat hastily chosen.

Another interesting feature in the fossil plants under consideration is the reappearance, at the far distant points from whence they come, of genera so well known in European and American geology, and the entire absence of the species of *Phyllotheca*, *Glossopteris*, etc., which have made the Indian and Australian coal floras so puzzling to the paleontologist. There are fragments of a new generic form, probably a Cycad, in the collection, and some obscure specimens that may represent other plants new to science, but the *Pecopteris*, *Sphenopteris*, *Podozamites*, *Pterozamites*, &c., have a very familiar look; and in their resemblance to well known forms give fresh evidence of the monotony of the vegetation of the globe previous to the introduction of the angiospermous forests of the Cretaceous period.

Whether the strata which have furnished these plants should be considered Triassic or Jurassic remains to be determined by future observations, as the fossils yet obtained can hardly be considered sufficient for the solution of that question.

From the "Kwii basin" we have numerous pinnæ of a species of *Podozamites*, undistinguishable from one found by Prof. Emmons in North Carolina in strata now generally regarded as *Triassic;* but associated with these are a few pinnæ of different form, much more elongated and acute, scarcely differing from those of a European Jurassic species (*P. lancolotus* Lind.). Still, the evidence of identity is much stronger in regard to the former species than the latter.

From Piyunsz we have a fine *Pecopteris* with the falcate pinnules so characteristic of the Mesozoic species, and, indeed, very accurately copying the form of *P. Whitbiensis*, a European Jurassic species; but unfortunately the strata which contain this fossil have been much metamorphosed, the coal converted to anthracite, and the nervation of the fern has been entirely obliterated, while the outline remains distinct.

Probably it will be found as difficult—or rather as impossible—in China, as it has proved in this country, to identify all the subdivisions of the Mesozoic strata discernible in Europe. Yet we shall doubtless gather there new proofs of the constancy of the order of sequence in geological history, and new evidence of the stability of the foundations on which geology as a science rests.

I have under my eye, as I write this letter, four collections of fossil plants, which, though from very widely separated localities, are curiously linked together. They are,—

1st. Fossil plants,—Cycads and Conifers,—collected by myself from the "Gypsum Formation" (Triassic) at Abiguiu, New Mexico. Of this collection the most conspicuous and interesting plant is *Otozamites Macombii* N.

2d. A collection of fossil plants—Cycads and ferns—received through Prof. J. D. Whitney from Sonora, Mexico, where they occur with coal strata and Triassic mollusks.

In this collection *Otozamites Macombii* is associated with *Strangerites magnifolia* Rogers, *Pecopteris falcatus* Emm., and other plants occurring abundantly in North Carolina.

3d. A collection of fossil plants—Cycads and ferns—from N. Carolina and Virginia, including, besides the last two mentioned, and many which are new, several species apparently identical with European Triassic plants, of the genera *Haidingera*, *Gutbieria*, *Laccopteris*, &c.; and among other Cycads, *Podozamites Emmonsii* N.

4th. The collection made by yourself in China—Cycads and ferns—in which one of the most distinctly marked plants is *P. Emmonsii.*

In regard to the American localities cited above, there is perhaps no good reason for our withholding assent to the conclusion that the rocks furnishing the fossil plants, are Triassic, but when we remember how much difference of opinion there has been, and indeed still is, upon this subject, even in the light of large collections of fossils, we can hardly with propriety offer even a conjecture as to the *precise* age of the Chinese coal strata.

To recapitulate. One species of *Podozamites* contained in the collection is apparently identical with an American Triassic species; the other more resembles a European Jurassic plant. The *Pterozamites* resembles both Triassic and Jurassic species, but is identical with neither. The *Pecopteris* has certainly a remarkable likeness to *P. Whitbiensis*, which occurs both in the Liassic and Oolitic floras; and it is not yet certain that it is not also found in the Carolina and Richmond coal basins.

The *Sphenopteris* and *Hymenophyllites* are altogether new, and suggest no affinites of value in this connection—while the *Taxites*, *Equisetites*, &c., are too obscure to afford us any help.

Cleveland, Ohio, Sept. 25, 1865.

ART. XXIII.—*A Second Method of correcting Monthly Means for the unequal length of the Months*; by ERASTUS L. DEFOREST.

IN the May number of this Journal I gave a system of twelve equations for finding the mean temperature, rain-fall, &c., for any *mean month* in terms of the means for the three nearest calendar months. This was done on the supposition that the curve of daily temperatures for any three consecutive months may be represented by a portion of a parabola whose equation is

$$y = a + bx + cx^2.$$

A similar system of equations may be found by assuming that the curve is of the form

$$y = a_0 + a_1 \sin(x - e_1),$$

which may be written

$$y = \mathrm{A} + \mathrm{B} \cos x + \mathrm{C} \sin x.$$

Let n_1, n_2, n_3, x_1, and c, retain the same significations which they had in my former article, only instead of denoting days, let them denote the proportional arcs, the days being reduced to arc in the ratio of 365¼ days to 360°. By a process of integration similar to that followed before, we shall find that

$$m_1 = \mathrm{A} + \frac{1}{n_1} \mathrm{B}[\sin(\tfrac{1}{2}n_2 + n_1) - \sin \tfrac{1}{2}n_2] - \frac{1}{n_1} \mathrm{C}[\cos \tfrac{1}{2}n_2 - \cos(\tfrac{1}{2}n_2 + n_1)],$$

$$m_2 = \mathrm{A} + \frac{2}{n_2} \mathrm{B} \sin \tfrac{1}{2}n_2,$$

$$=\mathrm{A}+\frac{1}{n_3}\mathrm{B}[\sin(\tfrac{1}{2}n_2+n_3)-\sin\tfrac{1}{2}n_2]+\frac{1}{n_3}\mathrm{C}[\cos\tfrac{1}{2}n_2-\cos(\tfrac{1}{2}n_2+n_3)],$$

$$=\mathrm{A}+\frac{2}{c}\mathrm{B}\cos x_1\sin\tfrac{1}{2}c+\frac{2}{c}\mathrm{C}\sin x_1\sin\tfrac{1}{2}c.$$

the above be written for brevity.

$$\begin{aligned}m_1&=\mathrm{A}+\mathrm{B}b_1+\mathrm{C}c_1,\\ m_2&=\mathrm{A}+\mathrm{B}b_2,\\ m_3&=\mathrm{A}+\mathrm{B}b_3+\mathrm{C}c_3,\\ \mathrm{M}_2&=\mathrm{A}+\mathrm{B}b''+\mathrm{C}c''.\end{aligned}$$

ninating A, B and C, and employing K and L as auxiliary rs, we shall find this expression for M_2:

$$\mathrm{K}=\frac{c''(b_2-b_3)-c_3(b_2-b'')}{c_3(b_2-b_1)-c_1(b_2-b_3)},$$

$$\mathrm{L}=\frac{c_1(b_2-b'')-c''(b_2-b_1)}{c_3(b_2-b_1)-c_1(b_2-b_3)},$$

$$\mathrm{M}_2=m_2+(\mathrm{K}+\mathrm{L})m_2-\mathrm{K}m_1-\mathrm{L}m_3.$$

may now proceed to compute for each of the twelve months arately, the numerical values of b_1, b_2, b_3, c_1, c_3, b'' and c'', from them the numerical coëfficients K and L, with the fol-ing result:

$$\begin{array}{llll}
\mathrm{M}_1=m_1 & +\cdot0036\,m_1 & +\cdot0031\,m_{12} & -\cdot0067\,m_2\\
\mathrm{M}_2=m_2 & -\cdot0127\,m_2 & -\cdot0030\,m_1 & +\cdot0157\,m_3\\
\mathrm{M}_3=m_3 & +\cdot0028\,m_3 & -\cdot0248\,m_2 & +\cdot0220\,m_4\\
\mathrm{M}_4=m_4 & -\cdot0042\,m_4 & -\cdot0199\,m_3 & +\cdot0241\,m_5\\
\mathrm{M}_5=m_5 & +\cdot0016\,m_5 & -\cdot0217\,m_4 & +\cdot0201\,m_6\\
\mathrm{M}_6=m_6 & -\cdot0039\,m_6 & -\cdot0179\,m_5 & +\cdot0218\,m_7\\
\mathrm{M}_7=m_7 & +\cdot0025\,m_7 & -\cdot0199\,m_6 & +\cdot0174\,m_8\\
\mathrm{M}_8=m_8 & +\cdot0025\,m_8 & -\cdot0103\,m_7 & +\cdot0078\,m_9\\
\mathrm{M}_9=m_9 & -\cdot0027\,m_9 & -\cdot0067\,m_8 & +\cdot0094\,m_{10}\\
\mathrm{M}_{10}=m_{10} & +\cdot0030\,m_{10} & -\cdot0085\,m_9 & +\cdot0055\,m_{11}\\
\mathrm{M}_{11}=m_{11} & -\cdot0026\,m_{11} & -\cdot0046\,m_{10} & +\cdot0072\,m_{12}\\
\mathrm{M}_{12}=m_{12} & +\cdot0032\,m_{12} & -\cdot0064\,m_{11} & +\cdot0032\,m_1
\end{array}$$

comparison of this system of equations, found by using a onometrical curve, with the former system which was found using an algebraic curve, is interesting as showing how far values of the numerical coëfficients are independent of the ure of the curve employed. The fact that these two sets of fficients differ but little, tends to establish the general accu- of both methods, and gives a high degree of probability to results derived from them. These results are almost iden-l. For the climate of St. Paul, where the great range of perature will of course magnify any small discrepancies, the an temperatures for mean months are by the first method of uction

13·74	46·92	73·44	46·97
17·79	59·41	69·93	31·50
32·05	68·73	58·68	16·79

and by the second method they are

13·74	46·94	73·44	46·97
17·80	59·43	69·93	31·49
32·08	68·74	58·67	16·79

showing a maximum difference of only ·03.

The equation of the curve of daily temperatures throug the year is by the first method

$$y=44{\cdot}66+29{\cdot}86 \sin (x-104^\circ\ 41')+ \text{\&c.},$$

and by the second method it is

$$y=44{\cdot}67+29{\cdot}86 \sin (x-104^\circ\ 40')+ \text{\&c.}$$

If it is necessary to choose between the two systems, per the second may be preferred for reducing temperatures, bec the trigonometrical curve admits points of inflexion, whict parabola cannot have; so that the latter can hardly be sai represent very well the curve of temperatures in the spring autumn months, where it changes from convex to concav the reverse. Accordingly we see that for St. Paul the differe between the monthly means found by the two methods greatest in the spring and autumn months, and null at mid mer and midwinter.

May, 1866.

ART. XXIV.—*On a New Process of Organic Elementary Ana for Substances containing Chlorine;* by C. M. WARREN.

ORGANIC bodies containing chlorine—and probably those that contain bromine and iodine—may be analyzed by a pro analogous to that which I have already described for substa containing sulphur.[1]

As in that process, so also in this, the substance is burnt stream of oxygen gas, in the manner described in my first pa on organic Elementary Analysis.[2]

Similarly, also, as in the analysis of sulphur compounds, chlorine is absorbed and retained during the combustion, l suitable substance placed in the anterior end of the combus tube; this substance being subsequently removed, and the c rine determined therefrom in the usual manner. The carbon hydrogen, in either process, are determined from the same tion of the substance as the sulphur or chlorine, in a manner similar in other respects to that described for simple hydrocarbons.[3]

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[1] Proceedings of the American Academy, March, 1865; this Journal, Jan., xli, 40.

[2] Proceedings of the American Acad., 1864, p. 251; this Journal, xxxviii, 3

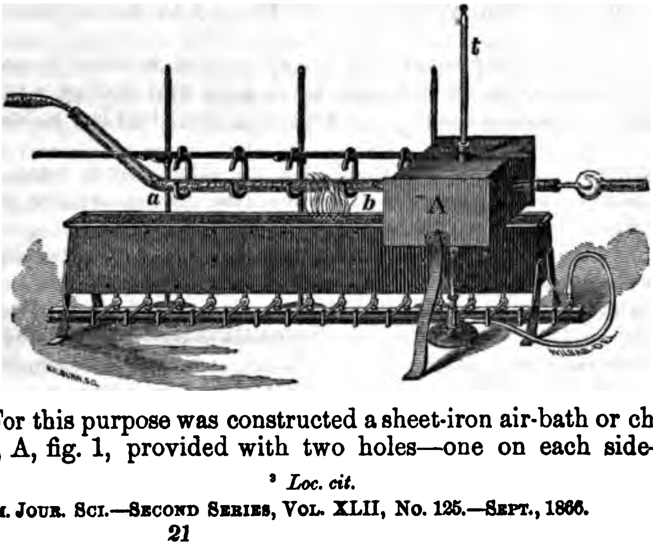
In pursuing this research some difficulty was experienced, as was anticipated, in finding a substance which would absorb and retain the whole of the chlorine, under conditions that would at the same time insure that every trace of the carbonic acid and water should pass through unabsorbed.

The search for this substance was confined to the oxyds of the heavy metals, as these alone, from their strong affinity for chlorine, and weak affinity for carbonic acid, seemed to give encouragement of success.

The difficulty, however, in finding such a substance was chiefly due to the circumstance that most of the chlorids of these metals are either too volatile, or begin to suffer decomposition at too low a temperature; it being requisite that the absorbing substance, and the newly formed chlorid of the same, should bear to be heated sufficiently to prevent both condensation of water and absorption of carbonic acid, and at the same time avoid a temperature high enough to occasion any appreciable decomposition of the chlorid.

This question of temperature became, therefore, a prominent one in the investigation, as evidently the success of the process must depend, in a great degree, on the proper management of the temperature of the absorbing substance, within such limits as might be found to give satisfactory results. Hence, my first step was to devise means to secure the necessary control of the temperature of that part of the combustion tube which should contain this substance.

1.



For this purpose was constructed a sheet-iron air-bath or chamber, A, fig. 1, provided with two holes—one on each side—to receive the combustion tube, and a tubulure in the top for a thermometer. One end of the air-bath is made to rest on the combustion furnace, and the other, which projects a few inches from the front of the furnace to make room for a lamp, is supported by a leg resting upon the table. The bulb of the thermometer is placed in a central position, in the interior of the bath, close by the side of the combustion tube.

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[3] *Loc. cit.*

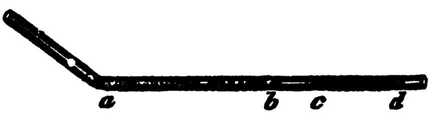
The temperature of the air-bath, and consequently of the substance contained in the combustion tube within, is easily regulated by means of a Bunsen's burner placed under the front end of the bath, as shown in fig. 1. With the exception of the air-bath, the apparatus employed is the same as that used in the analysis of substances containing sulphur, a full description of which is given in the papers above referred to.

The substance that I have found best adapted to absorb the chlorine, for substances easily combustible, is brown oxyd of copper, prepared by precipitation with potassa and ignition over a gas flame.

Difficultly combustible substances, like chloroform, are not completely burnt in oxygen in contact with asbestos alone, but require the presence of a body having affinity for chlorine; otherwise there is formed a liquid body, difficultly volatile,—probably a chlorid of carbon,—which condenses in the vacant part of the tube, from *b* to *c*, fig. 2, and which cannot be entirely burnt off and save the analysis. In such cases the absorbing substance is mixed with the asbestos occupying the back part of the tube, where the combustion takes place. It is evident that oxyd of copper would not answer for this purpose, as at so high a temperature dichlorid of copper would be formed, which, being insoluble in dilute acids, would interfere with the determination of the chlorine. Oxyd of zinc has been found to give good results with such substances.

The preparation of the combustion tube, and the arrangement of the mixture of asbestos and the absorbing substance, is the same—except in the case last mentioned—as in the analysis of substances containing sulphur, as shown in fig. 2, viz., the space between *a* and *b*, about 10 inches in length, is packed with pure asbestos; between *b* and *c*,—a space of about two inches,—being left vacant, a plug of asbestos is placed at *c*; the space between *c* and *d*, 4 to 5 inches in length, is filled with an intimate mixture of asbestos and brown oxyd of copper; and, finally, a plug of asbestos is placed at *d*.

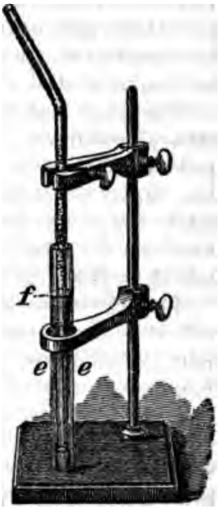
2.



After the combustion, the chlorid, together with the excess of oxyd, is extracted from the asbestos by means of dilute nitric acid.

To facilitate the removal of what may adhere to the sides of the tube, the apparatus shown in fig. 3 will be found serviceable as in the analysis of sulphur compounds.

3.



I. *Experiments with Oxyd of Lead and with Oxyd of Copper, placed in the anterior end of the combustion tube, as absorbents of Chlorine in the analysis of substances difficultly combustible.*

The substance selected for analysis, as a test of the process for that class of bodies which are difficultly combustible, containing but a small percentage of hydrogen, was commercial chloroform. The preparation employed was first subjected to redistillation. Its boiling point was found to agree essentially with that assigned to pure chloroform in Gerhardt's *Traité de Chimie.* When the usual tests were applied, no impurity could be detected.

Experiment 1.—A mixture of oxyd of lead and asbestos was placed in the anterior end of the combustion tube, between *c* and *d*, fig. 2, as previously described. As chlorid of lead was supposed to bear a pretty high temperature, without volatilization or decomposition, the use of the air-bath was omitted in this experiment, and the oxyd gently heated with a small flame from the combustion furnace. The combustion had not proceeded far when it became apparent, from deposition of minute drops of liquid on the sides of the vacant part of the tube,—from *b* to *c*, fig. 2,—that the combustion of the chloloform was incomplete, although no doubt could exist as to the presence of an excess of oxygen. This deposit of liquid, which, as already stated, was supposed to be a chlorid of carbon, was found to be difficultly volatile, suffering partial decomposition, and leaving on the tube a brown deposit, which was not entirely removed by ignition in a stream of oxygen. The high temperature employed to burn off this deposit occasioned excessive heating of the posterior end of the mixture of lead oxyd and asbestos; and this may have been the cause, to some extent, of the excess in the determinations of carbon and hydrogen, although subsequent analyses indicate that the sample of chloroform under examination contained a larger percentage of these elements—particularly of the latter—than belongs to pure chloroform. This experiment gave 11·47 per cent of carbon, and 1·87 per cent of hydrogen. Theory gives 10·07 per cent of carbon, and 0·85 per cent of hydrogen. The mixture of asbestos and oxyd and chlorid of lead was removed from the tube, and treated in the usual manner with a solution of bicarbonate of soda to obtain a soluble chlorid. This operation was found extremely tedious. Even after treatment for more than two weeks, with occasional fresh portions of the bicarbonate and frequent agitation, the decomposition of the lead chlorid was still found to be incomplete, and the operation was abandoned. As this is given in the text books as a good process for the separation of chlorine from chlorid of lead,[4] I am led to presume that in this case the excess of heat employed gave rise to the formation of an oxychlorid, which is, doubtless, more slowly acted upon by the bicarbonate. This single experiment does not, therefore, prove that oxyd of lead may not be employed in this process with good results, when used for easily combustible substances, and excessive heat is avoided. But it will, unquestionably, be found preferable to use a substance which will give *directly* a soluble chlorid.

Experiment 2.—This experiment was conducted as the last, with only this difference, viz. that oxyd of copper was substituted for the oxyd of lead. No better results, however, were obtained. The reappearance of the difficultly volatile liquid in the vacant part of the tube, while there was assurance of there being no deficiency in the supply of oxygen, served to confirm the impression gained by the preceding experiment,—that chloroform could not be completely burnt in oxygen alone, but that a substance having affinity for chlorine would have to be mixed with the asbestos, at the point where the combustion takes place.

II. *Experiments with Oxyd of Zinc, mixed with the asbestos in the posterior part of the combustion tube, as absorbent of Chlorine in the analysis of substances difficultly combustible.*

As already indicated, the chief object of this set of experiments was to determine whether the presence, at the point where combustion takes place, of an oxyd capable of combining with the chlorine would have the effect to prevent the formation of the difficultly volatile liquid above mentioned, and thus remedy that defect in the process.

Experiment 1.—In this experiment, three grams of oxyd of zinc were intimately mixed in a mortar with the quantity of asbestos necessary to fill the space between *a* and *b*, fig. 2, and that part of the tube then packed with this mixture in the usual manner. A similar mixture composed of asbestos and only one gram of oxyd of zinc was placed between *c* and *d*. The space between *b* and *c* was left still vacant, in order to be able to observe the effect. On account of the volatility of the chlof zinc, it was deemed advisable to retain the use of the air- to control the temperature of the anterior portion of the bustion tube, which, in this experiment, was not allowed to ed 160° C. The result was, as anticipated, that no such lensation of liquid between *b* and *c* occurred. In order to from this experiment some idea of the degree of volatility hlorid of zinc under such circumstances, the two columns sbestos were treated for chlorine, separately. The solution ined from the anterior column was found to contain but a e of chlorine, giving only a milkiness with nitrate of silver; ving that the chlorid of zinc does not travel far through a mn of asbestos from the point where the flame plays directly he tube.

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[4] H. Rose, Chimie Analytique, new French edition, p. 801.

esults of the Analysis.—0·2067 gram of chloroform gave 98 of carbonic acid, 0·0276 of water, and 0·7372 of chlorid ilver.

		Calculated.		Found.
Carbon	C_2	12	10·0671	10·5273
Hydrogen	H	1	0·8473	1·4514
Chlorine	Cl_3	106·2	89·0856	88·0455
			100	100·0242

xperiment 2.—In this experiment, the whole length of the bustion tube from *a* to *d* was packed with a mixture of as-os and four grams of oxyd of zinc. The temperature of the rior end of the combustion tube was regulated, as in the ious experiment, by means of the air-bath.

esults of the Analysis.—0·1339 gram of chloroform gave 0·0506 arbonic acid, 0·0156 of water, and 0·4768 of chlorid of silver.

		Calculated.		Found.
Carbon	C_2	12	10·0671	10·3062
Hydrogen	H	1	0·8473	1·2733
Chlorine	Cl_3	106·2	89·0865	87·9014
			100	99·4809

hese two analyses, agreeing as they do so closely, indicate the chloroform analyzed contained larger percentages of on and hydrogen,—especially of the latter,—and a corres-dingly smaller percentage of chlorine than the theoretical ntities; occasioned, probably, by the presence of some impu-. This view is supported by calculations made on the as-ption that the excess might have arisen from volatilization hlorid of zinc, or from incomplete absorption of the chlorine; ch would make the chloroform contain from two to six per t more than the theoretical quantity of chlorine. These lts are regarded, therefore, as satisfactorily establishing the ity of this process in the analysis of chloroform. But the analysis of this body, containing as it does eighty-nine per cent of chlorine, and only eighty-five hundredths of one per cent of hydrogen, must be considered as an extreme case, and does not prove the process a good one for other classes of substances.

The next step, therefore, was to determine whether the process would be equally efficient in the analysis of substances rich in hydrogen, the combustion of which would give rise to the formation of a large quantity of hydrochloric acid. The substance selected for analysis, to settle this question, was chlorid of amyl.

III. *Experiments with Oxyd of Zinc, as an absorbent of Chlorine in the analysis of substances rich in Hydrogen.*

In these experiments the oxyd of zinc was employed in the same manner as above described for the analysis of chloroform. The chlorid of amyl, which was the subject of analysis, was prepared in the usual manner. Its boiling-point was 102°, 8 corrected.

The following results of two analyses with oxyd of zinc indicate that this oxyd combined with and retained some of the carbonic acid. This result was not anticipated, as in the analysis of chloroform the determination of carbon was uniformly slightly in excess.[6]

The results of these two analyses are as follows:—

1.—0·1922 gram of chlorid of amyl gave 0·3513 of carbonic acid, 0·1854 of water, and 0·2528 of chlorid of silver.

		Calculated.		Found.
Carbon	C_{10}	60	56·3910	49·85
Hydrogen	H_{11}	11	10·3383	10·72
Chlorine	Cl	35·4	33·2707	32·47
			100·	93·04

---

[6] Since the above was written, I have observed upon reviewing my notes,—not only of experiments with oxyd of zinc, but also with oxyd of copper, that in every analysis in which I made note of carbonization, or blackening of the asbestos in the combustion tube,—which may sometimes occur from too rapid distillation of the substance, or, what amounts to the same thing, a deficiency in the supply of oxygen,—there was a loss in the determination of the carbon, and generally, also, in that of the chlorine; while the hydrogen would agree pretty nearly with the theoretical quantity. I am, therefore, at the present writing, inclined to suspect that the carbonization may have had some connection with the deficiency in the carbon determinations in these instances, although the blackening would readily and completely disappear as soon as a sufficiency of oxygen was supplied. The momentary blackening of the asbestos occurred in both of the analyses of chlorid of amyl with oxyd of zinc, but, as already intimated, was not regarded at the time of serious consequence, as similar phenomena in the analysis of hydrocarbons by my process were generally attended with good results. It may, therefore, remain an open question, whether the oxyd of zinc may not serve a good purpose in the analysis of substances of the class now under consideration.

2.—0·1657 gram of chlorid of amyl gave 0·3314 of carbonic acid and 0·1608 of water.

		Calculated.		Found.
Carbon	C_{10}	60	56·3910	54·56
Hydrogen	H_{11}	11	10·3383	10·74
Chlorine	Cl	35·4	33·2707	

IV. *Experiments with Oxyd of Copper, as absorbent of Chlorine in the analysis of substances rich in Hydrogen.*

In these experiments, for the reason previously stated, the oxyd of copper could only be placed in the anterior end of the combustion tube, where it might be maintained at a tolerably low temperature. After two or three experiments,—which were but partially successful,—it became apparent that the range of temperature within which oxyd of copper could be made serviceable to absorb the chlorine was probably rather limited.

It was observed, for example, that at 150° to 160° even brown oxyd of copper, which had been but gently ignited, would fail to absorb nearly all the chlorine, and consequently the determination of the carbon, and sometimes that of the hydrogen, would be in excess. In one experiment, in which the oxyd of copper was kept at about 153° C., its appearance had suffered no change, and it was found to contain only 8·29 per cent of chlorine, or only about one quarter of the theoretical quantity. When a sufficiently high temperature is employed, on the contrary, the posterior end of the column of oxyd of copper and asbestos has the appearance of being entirely changed into yellow chlorid of copper, the rest of the column remaining, for the most part, of its original dark color.

In another experiment, with the oxyd of copper kept at a temperature of about 160°, only about fourteen per cent of chlorine was obtained.

In both of these experiments the carbon determination was considerably in excess, and in one of them the hydrogen also. The oxyd of copper employed had been strongly ignited.

Before proceeding further with these somewhat random experiments, it was deemed advisable to *determine* the temperature at which chlorid of copper begins to give off chlorine, in order to know how far it would be safe to raise the temperature of the air-bath in conducting an analysis. By making use of the air-bath to regulate the temperature of the chlorid of copper, this determination was easily made. During the heating of the chlorid, a current of air from the air-gasometer was admitted through the tube in which it was contained.

Observations.—At 243° not a perceptible trace of chlorine was given off. After the lapse of fifteen minutes, at 250°, the nitrate of silver into which the gas was conducted, was observed to be slightly milky; this may, therefore, be taken as about the temperature at which chlorid of copper begins to suffer decomposition. At 267°, a solution of nitrate of silver was instantly precipitated.

Thinking that perhaps the small quantity of chlorine evolved under these circumstances might be taken up again and retained if oxyd of copper were present, and possibly, also, that in that case a higher temperature might be safely employed,—to make the conditions of the experiment conform in this particular to those which exist in an analysis, all but one inch of the chlorid of copper was removed from the tube, and in its place was put a mixture of asbestos and oxyd of copper, occupying a space of four inches in length, forward of the chlorid. The experiment was then repeated. Prolonged heating in a current of air, and afterwards in oxygen, during which the thermometer rose to 350°, produced no reaction with nitrate of silver. From this it appears that the chlorine, which was given off below this temperature from chlorid of copper, when this is mixed with oxyd of copper, is absorbed and retained by the latter; hence, that so high a temperature as 350° may be safely employed for the air-bath in conducting an analysis by this process.

Analysis 1.—In this analysis the oxyd of copper employed was prepared in the ordinary way and strongly ignited. The space in the tube occupied by the mixture of asbestos and oxyd of copper was five inches in length, and contained just five grams of the oxyd. During the experiment, the temperature of the air-bath was maintained at about 350°. At the close of the combustion there was no appearance of chlorid of copper, except in the first half-inch at the back end of the column of the mixture of oxyd of copper and asbestos; showing that the temperature employed was favorable for rapid and complete absorption of the chlorine.

Results of the Analysis.—0·1682 gram of chlorid of amyl gave 0·3486 of carbonic acid, 0·1633 of water, and 0·2233 of chlorid of silver.

		Calculated.		Found.
Carbon	C_{10}	60	56·3910	56·522
Hydrogen	H_{11}	11	10·3383	10·761
Chlorine	Cl	35·4	33·2707	32·773
			100	109·056

Analysis 2.—The oxyd of copper employed was of the same preparation as that used in Analysis 1. The space occupied by the mixture of asbestos and oxyd of copper was only 3½ inches in length, but contained the same quantity, viz. 5 grams of the oxyd of copper, as used in the previous analysis. The temperature of the air-bath ranged from 250° to 253°. At the close he combustion, it was found that all but ¾ inch at the ford end of the column of mixed asbestos and oxyd of copper the appearance of containing chlorid of copper. By comson with the corresponding observation in Analysis 1, it will een that the appearance of the chlorid extends over more five times the space in this analysis that it did in the former, wing that with strongly ignited oxyd of copper a temper-e higher than 250°, even as high as 350°, is more favorable the absorption of the chlorine. The following results of the lysis, however, are equally accurate with those of the preng analysis.

·1669 gram of chlorid of amyl gave 0·3457 of carbonic acid, 12 of water, 0·2213 of chlorid of silver.

		Calculated.		Found.
Carbon	C_{10}	60	56·3910	56·489
Hydrogen	H_{11}	11	10·3383	10·785
Chlorine	Cl	35·4	33·2707	32·732
			100	100·006

nalysis 3.—Under the impression that an oxyd of copper ch had been less strongly ignited might be effectual to absorb chlorine at a lower temperature, I employed in this and the following analyses a preparation of brown oxyd of copper, ined by precipitation with potash and ignition over an ordiy gas flame. In this analysis the temperature of the air-bath ged from 150° to 158°. The space occupied by the asbestos ture was four inches in length, and contained three grams the oxyd. Although the results of the analysis indicate that temperature of the air-bath was too low, they also show, by parison with the results obtained in operating with strongly ited oxyd at about the same temperature of the air-bath (see 63), that the brown oxyd is decidedly preferable in respect to temperature required. This was also shown by the appeare of the oxyd after combustion,—the newly formed chlorid ng confined, in the case of the brown oxyd, to a much shorter ce.

Results of the Analysis.—0·1640 gram of chlorid of amyl gave 504 of carbonic acid, 0·1562 of water, and 0·1884 of chlorid silver.

		Calculated.		Found.
Carbon	C_{10}	60	56·3910	58·268
Hydrogen	H_{11}	11	10·3383	10·582
Chlorine	Cl	35·4	33·2707	28·360
			100.	97·210

Analysis 4.—Used the same preparation of oxyd of copper as analysis 3, viz., the brown oxyd. Temperature of the air-bath reached 170°. Slight carbonization occurred just at the close of the combustion, from extending the heat backward too soon, under a wrong impression that the substance was all burnt. Were it not for this circumstance, it is believed that this would have been a good analysis, although the temperature of the air-bath was kept so low. That a higher temperature of the bath is desirable, however, is shown by the fact that the chlorid of copper appeared diffused over a space of 2½ inches. The length of the column of mixed asbestos and oxyd of copper was only four inches in this experiment, containing *but one gram* of the oxyd.

Results of the Analysis.—0·1568 gram of chlorid of amyl gave 0·3195 of carbonic acid, and 0·1522 of water.

		Calculated.		Found.
Carbon	C_{10}	60	56·3910	55·574
Hydrogen	H_{11}	11	10·3383	10·784
Chlorine	Cl	35·4	33·2707	

Analysis 5.—The oxyd of copper employed was of the same preparation as that of analyses 3 and 4. The temperature of the air-bath, however, was considerably higher, ranging from 240° to 247°. The mixture of asbestos and oxyd of copper occupied a space of five inches in length, but contained only two grams of the oxyd. At the close of the combustion there was no appearance of chlorid of copper, except at the back end of the column, a space ¾ of an inch in length.

Results of the Analysis.—0·1631 gram of chlorid of amyl gave 0·3383 of carbonic acid, 0·1557 of water, and 0·2157 of chlorid of silver.

		Calculated.		Found.
Carbon	C_{10}	60	56·3910	56·542
Hydrogen	H_{11}	11	10·3383	10·607
Chlorine	Cl	35·4	33·2707	32·649
			100·	99·798

It can hardly have escaped observation, that the quantity of oxyd of copper or oxyd of zinc required to absorb the chlorine by this process is extremely small, in consequence of its being uniformly diffused through a large mass of asbestos; hence it is obvious that but little of a solvent is needed to extract the chlorid. In this respect the new process bears a striking contrast to the old one, which involves the use of a large quantity of lime, necessitating a corresponding quantity of acid, and introducing disagreeable manipulation, which tend to increase the liability to error.

I have not yet tried the process recently described by Carius,* as the difficulty which I had found in obtaining tubes that would bear the pressure incident to his process for the determination of sulphur gave no encouragement of better success in the use of his process for the determination of chlorine, which is performed in a similar manner, although more complicated.

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* Annalen der Chimie und Pharmacie.

The advantage which my process affords, of being able to determine the three elements, carbon, hydrogen, and chlorine at a single combustion, without the introduction of any difficult or hazardous manipulation, induces the belief that it will be found preferable to any other that has been devised.

Art. XXV.—*The Vowel Elements in Speech;* by Samuel Porter, of Hartford, Conn.

The division of the alphabetic elements into vowels and consonants is one which grammarians have ever been compelled to recognize, however hard they may have found it to mark the distinction by satisfactory definitions. The nature of the vowels is such, somehow, that every word must contain at least one of them. The same is true, for the most part, of syllables as well as words. The consonants *l*, *n*, *r*, *m*, do indeed occasionally take the place of a vowel in a dependent, unaccented syllable, leaving the written and originally spoken vowel silent,—as ab*l*e, trif*l*e, id*l*e, harde*n*, mutto*n*, recko*n*, heave*n*, Britai*n*, ofte*n*, permit, fo*r*lorn, cu*r*tail, the*m*selves, give'e*m*, and the like,—sometimes properly, and sometimes by a slightly incorrect pronunciation. But no word, and no syllable under a full accent, is without a vowel. The rare exceptions which may occur, as in some of the Slavic tongues and the *ṛ* of the Sanskrit, cannot be allowed to invalidate the general rule. The difference between vowel and consonant is a fundamental one: the obvious difference in function rests upon a difference in essential nature,—what that is will be developed, in the sequel, as incidental to our main purpose.

Grammarians, at this day, are generally agreed as to the importance of what they call the physiological analysis of the alphabetic elements. The positions, motions, &c., of the vocal organs, as producing the sounds, need to be ascertained in order to a successful investigation of those relations of the phonetic elements which appertain to the science of language. The *Lautlehre* is understood to involve consideration of the *physiologische Process.* The term *physiologico-mechanical* would express the full idea more accurately than *physiological.* The phrase "mechanism of speech" may well denote the objective matter of inquiry. It is upon mechanical relations among the vocal elements that the laws of syllabication and euphony and the processes of phonetic transformation depend far more than on auditory impressions. Without a true physiological analysis, investigation into the laws of phonetic change must be merely empirical: only so far as an approximation thereto is realized, can comparative philology fairly claim rank as a science. In the matter of a "standard alphabet," and for the phonetical description of strange languages, as well as, finally, for orthoëpical and elocutionary purposes, the importance of such analysis will hardly be questioned. To one who should derive his ideas on the subject from *Le Bourgeois Gentilhomme* of Molière, such studies might indeed seem idle and ridiculous; but those who know anything of the subject see in it a matter of practical as well as scientific interest, sufficient to invite and to warrant the thorough and minute treatment which alone can yield valuable results.

As respects the vowels, such investigations have hitherto had but partial success. While there is a general agreement on many leading points, there is still on many others a wide diversity of views. Observations wanting in precision have led to a corresponding vagueness in the use of terms. Mere incidental concomitants have been mistaken for essential matters. Few systems have even professed such completeness as to find a place in an orderly scale for every vowel sound; none pretending to be thus complete has presented claims so demonstrably valid as to compel a general acceptance. Dr. Brücke, of Vienna, the author of a most thorough and able treatise on phonology, remarks that "the formation of the vowels still presents to us considerable theoretical difficulties, which it will take a long time perhaps to solve in a satisfactory manner."[1] Prof. Max Müller, in the second series of his Lectures, treats the physiology of the vowels with a good deal of particularity, but makes no attempt to present a complete and exhaustive scheme. In short, a true system of the vowels has thus far remained a desideratum.

The difficulties which beset the subject are considerable. In the first place, a special kind of practice with the elements, singly and separately, must have become familiar to the investigator. Not less essential is the careful training of the ear to the just discrimination of articulate sounds. Then, the experiments—to be made and observed again and again—will need much careful attention, and call for some ingenuity of contrivance. A slight variation of the "physiologic process," so slight as to be hardly perceptible, or not at all perceptible without observation of a special kind, will often result in a marked diference in the character of the sound produced. Attention will e needed to distinguish with invariable certainty compound lements from simple, mixed from pure; and especial care in rder to eliminate whatever is only incidental, or even purely ccidental, and so to seize upon what is really essential.

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[1] Cited at second-hand from Prof. R. L. Tafel's *Investigations into the Laws of English Orthography and Pronunciation* (New York, 1862).

When all is done so far, there remains the task of making ne's self understood by others. The clearest and most thorough xposition will be but labor lost upon those who have undisriminating ears and loose habits or incorrect modes of pronuniation; and the number of such among otherwise well-educated nen—linguists and grammarians with the rest—is by no means mall. Local and national diversities of pronunciation are nother barrier to a mutual understanding in these matters. he Teuton who speaks *cab* and *cub* nearly like *cap* and *cup*, *ag* like *back*, *food* like *foot*, with the *b*, *d* and *g*, as merely softned forms of *p*, *t*, *k*, after his own vernacular, even if at all ware that the sound of these letters is not precisely the same n German as in English, will certainly stumble at our physioogical analysis; and as it is with consonants, so with vowels; nly the difficulty is far greater.[1] We have in this country not few provincial, that is, local, or as we Americans say, sectional, eculiarities of pronunciation; and in Great Britain such exist n a more marked degree, in the case of the higher as well as the lower classes. Such diversities of usage, quite unsuspected it may be, are liable to render the examples employed for illustration ineffectual to any other result than a thorough misunderstanding.

I am led to offer my views on the subject because I believe that I have so far overcome the obstacles first named as to have hit upon the key to a true system of the vowels, and feel in duty bound to encounter the difficulties involved in the task of exposition.

I would not put forward my scheme in an attitude of antagonism toward the other systems or half-systems which have gained acceptance. I would have it regarded as completing what was fragmentary, and explaining what was but half understood,—by bringing to view certain new relations,—and as having its own substantial correctness confirmed by the ground it furnishes upon which to reconcile the conflicting diversities of other schemes. If, on minor points, my positions shall in any case appear open to question, or even be fairly convicted of inaccuracy, I shall be well content, provided I succeed in demonstrating the correctness of the system in its leading features. I have not, however, been careless of the details.

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[1] Baron Kempelen says, (Mechanismus der Sprache, 1791,) "there are there [in Germany] whole provinces where the people have never in their lives spoken a *b*, and cannot do so even for once." This is especially true of Saxony. A weakening, when not an entire falling off, of the muffled "sonant" quality of the medials, *b*, *d*, *g*, in whatever part of the word, is a very general characteristic of the Germans. Hence, their phonologists generally, and Max Müller with them, disallow this quality as distinctive, though Kempelen (a Viennese) strongly insisted on it as such, and illustrated it clearly by the experiment of a flageolet blown within a bladder.

The following principles are the key to the system:—

1. All the vowels are articulated primarily between the tongue and the palate. Some of them, those usually called labials (*o*ld, *oo*ze, *a*ll, &c.), are further modified by the action of the lips. All are thus either palato-linguals simply, or else labio-palato-linguals; and the latter consist of a palato-lingual part, capable of being employed by itself, and of a labial part which is dependent on and super-added to the other.

2. The articulation[a] is effected (1) as between the tongue and the palate in the following manner:—The organs are so disposed, and the muscles of the tongue, with those also of the soft-palate, so put into action, as to make a firm tube, or passage, fitted for the reverberation of the sound which comes from the larynx; and this passage so differs for all the vowels, as to modify the sound in a peculiar manner for each—the cases excepted, of course, in which the same palato-lingual articulation makes two distinct vowels as used with and without the labial modification. (2) The labial modification is effected by a firm contraction and more or less protrusion of the lips together with a rigid tension of the cheeks, so as to cause a further reverberation of the sound, and thus give the vowel a different character to the ear: the sound is reverberated through two passages or cavities instead of one.

3. The vowels—labial and non-labial—are assorted into *groups*, according as the palato-lingual passage extends more or less forward. The passage is either just at the throat, or is extended and lengthened by joining the lateral margins of the tongue to the sides of the palate, till finally the tube so formed reaches quite forward under the dome of the hard-palate and nearly to the tip of the tongue; though it is to be remarked that, for the anterior groups, the place is more precisely determined upon the palate than upon the tongue, owing to the extensile property of the latter. By "the throat" I mean the orifice bounded by the posterior extremity of the palate above and the root of the tongue below.

4. Each group thus determined embraces individual vowels

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[a] The words *articulate*, *articulation*, as applied to speech, point primarily to the *articuli*, small members or joints,—that is, the syllabic utterances,—of which speech is composed; but have been technically applied to the elements into which syllables themselves may be resolved; and by French Grammarians are usually restricted, without good reason, to the consonant elements; this is seldom done by English or German writers. It is further allowable to use the terms, as I do here, with particular reference to those mechanical adjustments of the organs which give to the several elements their distinctive character.

differing in *degree* as more or less open or close. These differences are effected, in the palato-lingual passage, by approximatng more or less to the palate the part of the tongue at the place of the articulation, especially at the front terminus of the passage; the passage may at the same time be narrowed more or less, as more or less of the margin of the tongue is put into contact with the borders of the palate. The labials will need no other or further criterion; for, in their case, the more or less openness of the lips will correspond to that between tongue and palate.

The scheme does not contemplate a precise admeasurement of open and close as between vowels of different groups; it requires no such comparison; it deviates from other systems in this, especially, that it assigns to different places, and thus ranges under eparate groups, vowels which have been commonly viewed as differing merely in degree of openness.

The degree of openness as between tongue and palate is not only to be distinguished from the greater or less opening of the aws, but it is to be noted that the two do not always coincide, and, especially, that the close labials (*awe*, *owe*, *oo*ze, &c.,) involve a wider separation of the jaws than the corresponding open and non-labial vowels (n*o*r, n*o*t, f*u*lly, &c.). A decided labial modification requires, absolutely, a considerable opening of the jaws, that the stretched cheeks may wall the passage.

A glance at the diagram and the table a few pages forward, will give the reader a more definite general idea of the scheme. *Some preliminary points* require attention before proceeding with the details.

1. As to the *number of vowels capable of being produced*, there is no certain limit in nature. The variations in degree of openness are obviously infinite; the variations as the terminus of the palato-lingual passage is more or less in advance are in like manner infinite; and each variation causes a greater or less difference in the quality or character of the sound. Thus, to say nothing of the labial modifications, and excluding diphthongs of course, we have, theoretically, an infinite multiplied by an infinite, for the number of possible simple vowel sounds. We have nothing which answers to the laws of melody and harmony in music whereby to determine the intervals of the scale. All we can do is to mark certain points, as if by lines of latitude and longitude, and make no account of intermediate gradations any further than to refer them to the nearest of these points. As for the number to be recognized in a system, much will depend on the special purpose in view. My object obviously requires a scheme both comprehensive and minute,—and will exact minuteness of orthoëpical detail in the way of illustration.

Languages vary greatly in the number of vowels they employ. In all our modern tongues we have many more than the three original vowels of the Sanskrit and the Gothic,—though we can hardly doubt that these three admitted severally considerable latitude of variation. In English, we recognize as distinct many more than we have separate characters for; while over and above these, we may notice slight variations, as due to the influence of associated consonants, or in connection with varying accentuation or emphasis; and, in the pronunciation of different persons, and even of the same person at different times, we observe appreciable shades of difference in what will be usually regarded as the same vowel.

2. In all the vowels alike, *the sound proceeds from the larynx*, being struck out upon the chords of the glottis, which, when drawn near each other to the proper interval and duly contracted, are set into vibration by air forced through from the lungs; the sound is then modified into this or that vowel by reverberation through a passage of this or that description. In vowels whispered, the vocal chords are so adjusted that we have aspiration, or breath-sound, instead of tone; but the sound originates in the larynx, and not, as in the consonant *f*, for example, at the place of articulation.

Sound produced in the larynx, intonated or aspirated, does not always take the form of a vowel. So it does not in the sound (*hm*) made in clearing the throat; in which case it undergoes a peculiar modification, but makes no vowel. Laryngeal sound is heard also in many of the consonants,—*l*, *m*, *r*, *v*, *z*, for example,—without being at all modified vowel-wise.

3. The larynx opens directly into *the pharynx*, which is a musculo-membranous sac through which the breath from the larynx must pass on its way to either the mouth or the nares, behind both of which the pharynx is situated, being properly a continuation of the esophagus, and separated by the *velum palati* from the mouth. The form and disposition of this organ will of course be affected by the movements and positions of the tongue and soft-palate; but we know not that the organ has any special agency in giving their distinctive character to the several vowel elements; at all events, its part must be quite a subordinate one. Inasmuch as the *velum palati* and the root of the tongue form the front wall of the pharynx, and as I hold that the vowel-tube begins always as far back as the throat, and that it is, moreover, essentially modified by varying adjustments of the *velum*,[4] so far my theory involves, indeed, a modification of the pharynx itself, and one that differs more or less for different vowels. That there is an action of the fore-part of the

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[4] See the positions of the soft-palate marked on the diagram. Upon this matter, my independent examinations led me to conclusions not differing from those established by the elaborate and ingenious experiments of Dr. Czermak, of Vienna.

pharynx in all the vowels, was observed by Prof. Leon Vaïsse, of Paris; and it was regarded by him as giving them that general character or quality in which they differ from the consonants.[1] My view is that the whole vowel-tube is concerned in producing this general character, by the very same action which gives distinctive character to each several vowel.

4. After the vocal current has passed the palato-lingual passage, it has still to traverse a further portion of the oral cavity; and in every case the sound will be in some degree modified or colored according to the disposition of those anterior parts; but, except in the proper labials, the effect will not amount to a change in the essential character of the vowel. The mouth may be more or less open, that is, the jaws and lips more or less far asunder, and yet the vowel be the same. The cheeks may even be tensely drawn close to the teeth, very much as in the more open of the labials, and still leave so predominant the essential character given by the palato-lingual passage, that we recognize the modification only as a different resonance given to the same vowel. In the word *hail*, for example, we should by such means give to the vowel a more full and sonorous quality in the recitation of "H*ai*l, holy light," than would be suitable in the expression, "H*ai*l-fellow, well-met." We can even utter and recognize the vowel *a*h, f*a*r, with the lips in position for f*oo*t or *oo*ze. The tongue, beyond and just forward of the proper terminus of the palato-lingual tube, may happen to flare or bend away but slightly, and so the observer be led to mistake the place of articulation as further forward than it is in fact, unless he correct himself by trial with different positions of the organ, which he should be careful to do.

5. Some who read this may need, at the outset, to be disabused of the habit,—arising from the customary use of the terms "long" and "short,"—of *confounding quantity*, or time, *with quality*, or character as respects articulation. Let it be observed then, that no amount of mere prolongation will change, for instance, the so-called short vowel in *full*, *pull*, *foot*, into the corresponding long in *fool*, *pool*, *rule*, *food;* nor will any process of curtailment convert the latter into the former. This is but an example of what is true universally of the vowels in the English, and to a greater or less degree in all languages. The tendency to variation of quality when a vowel is lengthened or shortened, is natural and universal.

Philologists have been accustomed to define the difference of long and short, in the vowels, as one of duration merely; but have found themselves forced—in a real inconsistency with this —to treat the distinction as one of fundamental importance in etymology: the differing relations of the long and of the short, which they must and do recognize, are quite unaccountable on their view of the case. If we lay open the physiological ground of a difference here in quality, we do so much to place etymological science upon the right basis.

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[1] See the article, PAROLE (Physiologie et Grammaire), from the pen of Prof. Vaïsse, in the Supplement to the *Encyclopédie Moderne* of the MM. Didot.

6. There are questions concerning *the relation of the vowels to tone or pitch.* Have the vowels each what may be called in any sense their natural pitch? This, if so, can help little to a knowledge of their proper vowel character, which remains the same under every variety of pitch. Is the peculiar character of each vowel to be explained as a certain combination of harmonic notes? This, if so, will not help much, in our physiological inquiries, till we have a better understanding of the mechanical conditions upon which such combinations depend in other cases. Prof. Max Müller, in reporting the discoveries of Helmholtz on this point, tells us the vowel quality is to be explained as exactly analogous to the *timbre* by which instruments, as the violin, flute, harp, &c., are distinguished one from another. (Lect. on the Sci. of Lang., 2nd ser., pp. 127–8: Am. ed.) But we have voices differing in *timbre,*—the reedy voice, the flute-like voice, &c., differing as do the instruments to which we liken them,—yet each voice uttering all the vowels, and giving to every one its peculiar character. We cannot, therefore, accept this as an adequate explanation. As I have not seen the work of Helmholtz, and as the account of his investigations which Müller gives is probably very imperfect, I shall express no opinion upon their merits or bearing. They run on a different line of inquiry from that which I have here in hand, and neither supersede it nor interfere with it.

There are two or three facts under this head, which, if not already familiar, can be easily verified. *First:* If we utter in succession any two or more of the simple vowel elements, we shall find them naturally taking a different pitch one from the other; and, referring to our series of groups arranged according to the place of articulation as reaching more or less forward upon tongue and palate, we find that, in passing from any vowel in the scale to one further forward and similar in degree of openness, the voice rises in pitch, while, proceeding in the other direction, it as naturally falls. Of this fact I have an application to make, by and by, in reference to what Dr. James Rush calls the *vanish* of the vowels. The following I propose as a physiological explanation of the fact. The movements of the tongue have an influence upon those of the larynx, through the intermediation of the hyoid bone, a moveable fulcrum with which both organs are connected by muscle and ligament. The connection is such that a movement of the tongue will require a reädjustment of the muscles of the larynx, to keep the latter organ in the same place and condition as before, and so to preserve the same pitch. Hence, though the two successive vowels *can* be uttered on the same pitch, a special intention will be required for it, and it is much more easy and natural to vary the pitch. *Second:* Every singer knows that only with certain vowels can the extremely high notes be fairly reached, and only with certain others again the lowest. The connection just mentioned between tongue and larynx will suffice to explain this also, though we would not affirm that there is no other cause concurrent therewith. *Third:* the vowels cannot each be uttered on every pitch with equal purity of tone, and there would seem to be one certain key for each, on which the purest tone is heard,—the purest musically we mean, that is, the most free from discordant intermixture. The same cause above mentioned may have an agency in this case also; but I think that here the effect may be due primarily to the form and dimensions of the vowel-tube.

7. I would have distinctly understood *what I do not, as well as what I do, affirm.* I do not say that the character of the several elements depends on the length of the vowel-tube, or on its shape, or the structure of its parts; I believe it, however, to depend on a combination of all these. I do not fail to perceive that, even in the anterior groups, the position and action of the tongue are, in each case, attended with a particular configuration and action of the soft-palate, which is important as affecting the quality of the vowel. I will not deny that the reverberation against the dome of the palate, at a point forward of the proper palato-lingual tube, may bear an important part in impressing upon some of the elements their proper character.[1] I state simply that the vowel-passage is made as I have described, and that the forward terminus of the palato-lingual part is in fact as I have indicated in each case.

I also recognize peculiar styles of enunciation as affecting somewhat the place of the articulation. There is the guttural style, retracting the throat and thus moving back the place of all the vowels. There is also the thin, dental style, just the opposite of the other. But, in each of these modes, the several vowels will be found to maintain their relative places, in substantial accordance with the scheme. I have aimed to assign their places, as definitely as possible, as they are in what I consider the most natural and least affected style of utterance, and as the places at which the peculiar quality of each element is most distinctly brought out and sharply discriminated.

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[1] Prof. Henry N. Day, who has given much attention to the subject, seems disposed to regard the peculiarities of the several vowels as proceeding wholly from this cause. See his article on *English Phonology* in the *Biblical Repository* for Oct. 1848.

In the *physiological analysis*, which I am now to give, *of the simple vowel elements*, I shall arrange them in *nine groups*,—according as the palato-lingual tube reaches farther or less far forward,—which I shall designate as the *a*, *å*, *o*, *u*, *ö*, *ä*, *e*, *é*, *i* vowels, or groups of vowels. Under each group I note *four degrees*, more or less close or open, which I call *close*, *middle*, *open*, *open-depressed*, and indicate by numerals affixed as superiors; thus: a^1 (close), a^2 (middle), a^3 (open), a^4 (open-depressed). Labials will be distinguished by an ^{l} affixed to the figure for the degree, as: o^{1l}, u^{1l}, $å^{2l}$, &c.

The diagram and the table, here inserted, depend of course, for their explanation, upon the analysis in detail which will follow. In the *diagram*, we have the median line of the palate from root of front teeth to root of uvula,—the hard-palate marked by the heavier stroke, and the soft-palate by dotted lines which indicate the varying configuration for different vowels, particularly *a*, *o* and *u*. The letters mark the termini of the palato-lingual tube as on the upper or palatal side; short dashes mark the termini below as on the median line of the tongue, and thus, of course, show the several degrees of openness,—they indicate, also, the direction of the vocal current at the termini.

Diagram of Palato-lingual Positions.

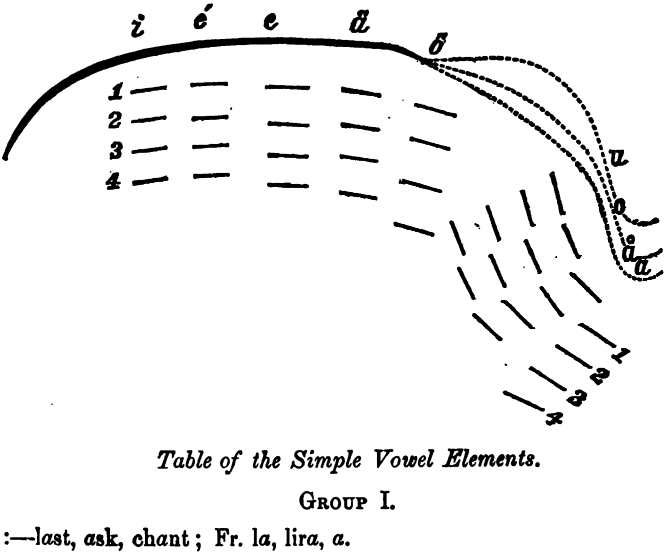

Table of the Simple Vowel Elements.

GROUP I.

a^1 :—last, ask, chant; Fr. la, lira, *a*.
a^2 :—father, calm; Fr. dame, malade, caver.
a^3 :—ah, arm, charge; Fr. âme, bas.
a^4 :—Broad pron. of psalm, balm, pass, &c.; do. of Fr. âme, bas.

Group II.

:—w*a*r, lord, *awe*, p*au*se.
:—*a*ll, w*a*ter, long, d*au*ghter; first element in bo*y*, vo*i*ce.
—s*a*lt, *a*lthough, cr*o*ss, h*o*rror.
—s*o*d, n*o*r, *o*ff, wh*a*t, kno*w*ledge.
—Low Ger. *a*; wrong pron. of w*a*r, lord, glory, forth, scorn, and of first elem. in jo*y*, rej*o*ice.

Group III.

:—main element in note, t*oe*, l*ow*, lo*a*f, door, m*ou*rn, b*eau*, h*au*tboy; Fr. ôter, *eau*, pivot; Ger. *O*fen, lobt, Mond.
—Wrong pron. óf note, t*oe*, &c.
:—*o*pinion, agony, propose, mell*ow*; Fr. *o*béir, noble, porter, m*o*t, dominer; Ger., kochen, Holbe, Morgen.
—Wrong pron. of c*oa*t, t*oa*d, stone, &c.
—not, dot, folly, knock, proper; Ger. Gott, flott, Ross.
—Wrong pron. of d*oo*r, *oa*r, b*oa*rd, &c.; Fr. encore, corps, alors, aurore.

Group IV.

:—f*oo*l, pool, moon, move, sh*oe*, s*ou*p; main and final elem. of *u*nion, f*ew*, vi*ew*, be*au*ty; Fr. r*ou*ler, v*ou*s; Ger. Sch*u*le, St*u*hl, g*u*t.
:—f*u*ll, p*u*ll, bosom, woman, sh*ou*ld, *good*, f*oo*t, book; final elem. in o*u*r, n*ow*, r*ou*nd; vanish of w*oe*, r*ow*, roll; Fr. c*ou*p, b*ou*t, b*ou*rse; Ger. l*u*stig, Sch*u*ld, B*u*nd.
:—Wrong pron. of pull, f*oo*t, book, &c.; Ger. d*u*rch, B*u*tte.
:—f*u*lfill, willf*u*l; wrong in f*oo*t, soon, p*u*t, &c.
:—Qu: Scotch g*u*de, s*u*ne, bl*ui*d, p*ui*r, (for *good*, *soon*, *blood*, *poor*,)? Initial of d*ew*, n*ew*, t*u*be, l*u*te, s*ui*t, r*u*de, &c.—u^4 or u^3.

Group V.

:—Ger. sch*ö*n, K*ö*nig, V*ö*gel; Fr. j*eû*ne, heur*eu*se, f*eu*x.
:—Ger. Wörter, möchte; Fr. l*eu*r, j*eu*ne, amat*eu*r.
:—mercy, virtue, girl, m*y*rtle, *ea*rl, pearl, *ea*rth.
:—*u*p, b*u*t, c*ou*sin, r*ou*gh, dove, done, fl*oo*d; Fr. d*e*, l*e*, c*e*, and (nasal) *u*n, br*u*n.
:—Fr. b*eu*rre, *coeu*r.
:—Broad pron. of ch*u*rch, work, &c; first elem. of *ou*r, bound, now, also of *i*ce, m*y*, r*i*ght.

Group VI.

:—Ger. M*ä*dchen, t*ä*glich, w*ä*re, g*ä*be, l*e*ben, g*e*ben, gel*e*gen; Fr. apr*è*s, sc*è*ne, pl*aie*, jam*ai*s, f*ai*re, p*è*re; Eng. th*ei*r, f*ai*r, p*a*rent.
:—c*a*re, th*e*re, pr*a*yer, h*ai*r, p*ai*r,—$\ddot{a}^2$ or $\ddot{a}^1$; Ger. r*ä*chen, d*ä*mmern.
:—*a*t, c*a*t, m*a*n, s*a*d, h*a*p.
:—Drawling of c*a*t, m*a*n, &c.; Fr. (nasal) v*i*n, f*i*n, cous*i*n.

Group VII.

:—Main elem. in f*a*te, n*a*me, gr*ea*t, v*ei*n, gr*ey*, h*ail*, p*ay*, g*ao*l, g*ua*ge; Ger. m*e*hr, j*e*der, l*e*dig, S*ee*; Fr. perhaps in some cases the "open *e*."
:—nitr*a*te, clim*a*te, parl*ia*ment, and usually initial in f*a*te, n*a*me, &c.; Ger. f*e*rtig, K*e*ller, Lieb*e*, Vat*e*r; Qu: Fr. *ai*mer, m*ai*son, &c.?
:—g*e*t, *e*gg, red, m*e*nd.
:—Qu: Fr. tête, bête, fête?

GROUP VIII.

é^1 :—Fr. bont*é*, cit*é*, j'*ai*, aimer*ai*.
é^2 :—guin*ea*, vall*ey*, carr*ie*d, cit*y*, and (vulgar) Americ*a*; Fr. c*e*tte, t*e*lle, ciel, *ai*mer, m*ai*son.
é^3 :—Ger. d*e*nn, B*e*tt; goodn*e*ss, coll*e*ge, &c.
é^4 :—Swedish long *é*, as in Carl*é*n.

GROUP IX.

i^1 :—mach*i*ne, f*ie*ld, *ea*t, *e*ve, d*ee*p; Fr. av*i*s, l*i*re, am*ie*; Ger. M*i*ne, m*i*r, w*i*der.
i^{1l}:—Fr. r*u*se, Gr*ue*; Ger. *ü*ber, Sch*ü*ler.
i^2 :—d*i*vine, veh*i*cle, mit*i*gate, mandar*i*n; the vanish of n*a*me, h*ai*l, &c., also of *i*ce, m*y*, &c., and of o*i*l, bo*y*, &c.; Fr. am*i*, f*i*dèle, f*i*er, v*i*f; Ger. m*i*t, b*i*tten, *n*icht.
i^{2l}:—Fr. *u*ne, r*u*de; Ger. Gl*ü*ck, w*ü*nschen. Initial of *u*nion, *view*.
i^3 :—p*i*n, h*i*t, s*i*n, w*i*ll.
i^4 :—Drawling of p*i*n, w*i*ll, &c.; initial in a Yankee pron. of d*o*, r*u*de, sm*oo*th, &c.

Physiological Analysis of the Vowel Elements.

I. THE *a* VOWELS.—For these, the place of articulation is between the root of the tongue and the extremity of the soft-palate, that is, at the throat. That no part of the tongue but the root is essentially concerned in the articulation, may be easily ascertained: for the tongue can be variously rolled and twisted without materially marring the vowel sound. The tongue may lie loose upon the floor of the mouth, except that the whole will naturally so participate in the movement of the root portion as to be somewhat raised in the close vowel. There are no labials in this group.

Degree 1.—*Vowel* a^1. This, the quite close vowel of the group, is proper in such words as st*a*ff, gr*a*ft, p*a*ss, *a*sk, l*a*st, ch*a*nt (Princ. of Pron.,"[7] §§ 5, 6.) It is the closest *a* in French, as in l*a*, lir*a*. It has a strong tendency to pass into ä^3 (c*a*t),—the natural position of the tongue and also that of the soft-palate being nearly the same for both,—but the two sounds are to be clearly discriminated.

Degree 2.—*Vowel* a^2. The proper Italian *a*, and the ordinary *a* in French, as ét*a*blir, m*a*l*a*de. In English, the Italian *a*, as we call it, in f*a*ther, *a*rm, &c., is variously heard, but this form is to be regarded as the more elegant in most cases.

Degree 3.—*Vowel* a^3. The open or broad *a* in French, as *â*me, b*a*s, gr*â*ce, p*a*ssant,—prolonged when under the circumflex.

Degree 4.—*Vowel* a^4. The open French *a* may by some be pronounced in this form, that is, with the utmost depression of the root of the tongue. So, also, the broad Low German *a*, and the Scotch broad *a*, in man, &c., though more commonly as ä^4.

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[7] *Principles of Pronunciation*, prefixed to the new edition (1864) of Webster's Dictionary.

In English, the *a* may sometimes be heard, improperly, thus broad and open, in words like ps*a*lm, b*a*lm, p*a*ss.

II. THE å VOWELS.—The posterior part of the tongue is somewhat raised and is adjusted on each side to the lower portion of the soft-palate to form the vowel-tube, which thus extends upward and forward a little way from the throat, and directs the vowel current obliquely upward. In this condition of the back-tongue, the tip and fore part will be naturally retracted, and more or less so as the vowel is more or less close.

A retraction of the tongue is natural also when the jaws are set widely apart for a very open *a* vowel; and this may be a special ground of the easy and frequent transition between vowels of that group and this, which actually occurs in the history of vowel changes.

Degree 1.—*Vowel* å^{1l}. W*a*r, w*a*rm, *awe*, l*o*rd, f*o*rm, *o*rder, p*au*se. (Princ. of Pron., §§ 7, 25.) The labial modification is decided, the lips being strongly contracted and protruded, and the cheeks drawn inward. Indeed, pure vowel-utterance in the very closest form is almost impossible without the lip-contraction. If we attempt it, we make an approach to the soft German *g* in *Tage*.

Degree 2.—*Vowel* å^{2l}. The difference between this and the preceding is not very strongly marked to the ear; but in some words the associated consonants make the vowel less close, as *a*ll, w*a*ter, w*a*nder, s*o*ng, sw*a*llow.

This vowel is the first element of the diphthong b*oy*, v*oi*ce.

Vowel å^2. Like the preceding except in the absence of the labial-modification: as s*a*lt, *a*lthough, h*o*rror, s*o*ft, s*o*lve, cr*o*ss, g*o*ne, c*au*ght. (See Prin. of Pron., § 21.) Let a practised elocutionist try such an example as, "All the horrors of war," which contains this and the two preceding vowels, and, his voice and ear being without fault, he will not fail to recognize each as different from the others.

Degree 3.—*Vowel* å^3. S*o*d, pl*o*d, n*o*r, *o*ff, wh*a*t, kn*ow*ledge; differing but slightly from the preceding.

Degree 4.—*Vowel* å^4. Here properly falls the broad Low German *a*. Here, also, we find the initial element in a certain flat pronunciation of j*oy*, rej*oi*ce, &c., heard not unfrequently,—å^4 in place of å^{2l}. Some speakers use this in place of the properly close vowel in w*a*r, *a*ll, l*o*rd, *awe*, &c., and even for the long *o* in glory, glorious, and other words.[*]

The Scotch mon, bl*aw*, sn*aw*, (for man, bl*ow*, sn*ow*,) obviously belong to the *á* group, but, to which degree, my opportunities have not been such as to enable me to determine.

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[*] Gardiner, in his *Music of Nature,* (p. 61, Am. ed.,) says of Macready ,"By aiming too much at distinctness, he incurs a false pronunciation of the vowels, which proceeds from his drawing back too much the corners of his mouth; so that we have *scarn* for scorn, *go farth* for go forth, *harrible! harrible!* for horrible! horrible!" *Drawing back the corners of the mouth* is identical with entire absence of labial modification; the vowel actually heard was, I doubt not, å^4.

It is to be remarked that the open-depressed degree suits in all cases with long quantity, and thus, in every group except the first (*a*), is liable to appear as a substitute for the close degree, which is naturally long in all but the first group, while the simply open degree (No. 3) is, with the same exception, the one least of all fitted for long quantity.

III. THE *o* VOWELS.—The palato-lingual passage is extended one step further, to a higher point along the *velum palati*; and, in direction, is inclined still more highly upward; the *velum palati* itself is higher and more arched. As the back-tongue is thus raised, the fore-part will naturally rise also. Like as in the preceding group, the tongue will naturally be retracted more or less according to the degree of closeness.

The close and open or middle *o* correspond to the long and short *o* in most of the languages of Europe; thus we have *o* close and long in our n*o*te, open and short in n*o*t. There is, however, in the Italian and some other tongues, especially the Danish and Swedish, a distinction of so-called open and close not identical with this; and to confound that with this would be a serious mistake. The Italian close *o* (*o* chiuso, stretto) is described by Dietz, A. J. Ellis, and others, as nearer to the *u* (r*u*de, f*u*ll). The Italian open *o*, (aperto, largo,) probably lies a little on the other side of our *o* and nearer to the *á*. The distinction, which holds alike in the long and the short vowels—cr*o*ce, b*o*cca (close), m*o*do, d*o*tto (open),—is at this day a nicety of pronunciation not generally recognized or regarded except in the purest style of the language as spoken by native Italians.

Degree 1.—*Vowel* o^{11}. N*o*te, *o*ld, *o*ver, &c., that is, the "long o"; French, tr*ô*ne, *ô*ter, rep*o*s, cl*o*s, dép*ô*t, piv*o*t, *au*, *eau*, &c.; German, *O*fen, l*o*bt, M*o*nd, &c.

The English "long *o*" is almost always diphthongal, or compound, taking a vanish in another vowel, which is commonly of the *u* group (f*u*ll, f*oo*d); as plainly appears in h*oe*, b*ow*, n*o*, b*ow*l, *owe*, l*ow*, &c. Followed by *r*, as in b*oa*rd, st*o*re, g*o*re, *oa*r, r*oa*r, the vanish is a labial vowel of the *ŏ* group (b*u*t, *e*rr, Fr. *eu*). In sp*o*ke, br*o*ke, *o*ver, als*o*, &c., quickly uttered, the vanish, if any, is imperceptibly slight. In English as spoken by foreigners, the long *o* without the vanish, in accordance with their own vernacular, is usually a noticeable peculiarity. In the customary English utterance, the main element is so disguised by the vanish, that its real character is not ordinarily observed, while the labial modification still further obscures its relation to the open vowel (deg. 3) of the group.

Vowel o^{1}. Sometimes heard as an improper mode of pronouncing the long *o* in English,—with the vanish, but without labial modification: one of the affectations of some public speakers. It might possibly be identified as an Irish or Scottish peculiarity, or both.

Degree 2.—*Vowel* o^{2l}. Heard in syllables which take a sort of secondary accent, as opinion, cotemporary, agony, mulatto, mellow, propose, proceed. And in a considerable number of words, such as stone, coat, toad, loaf, &c., the best taste will, perhaps, prefer this to the extreme close *o*, not neglecting, however, something of the vanish; also in torn, lorn, board, door, &c.

We have here the shorter *o* in French, as obéir, noble, &c. The German short *o* may, as I think, fall sometimes here and sometimes in the third or open degree.

Vowel o^{2}. An extremely improper pronunciation of a class of words just alluded to, coat, stone, toad, throat, whole, loaf, &c., quite common in America, and more especially in the rustic dialect of New England. The fault is commonly described as consisting simply in the omission of the vanish (Princ. of Pron., § 20), but the non-labial character and the more open degree are in fact equally essential. Board, door, *oar*, torn, &c., are also frequently and faultily so pronounced.

Degree 3.—*Vowel* o^{3}. Not, dot, hop, &c., which with those under $\mathring{a}^{3}$ (nor, off, sod, &c.,) are the "short *o*" in English. The distinction between o^{3} and $\mathring{a}^{3}$, though slight and usually not regarded by orthoëpists, is actually existent in practice, but depends mostly, we believe, on the influence of consonants associated.

Here, I think, belongs the shortest German *o*, Gott, Ross, flott; as also the French, sotte, culotte, folle, &c.

Degree 4.—*Vowel* o^{4}. Differs not greatly, but I think appreciably, from the $\mathring{a}^{4}$. Here belongs, if I mistake not, the French encore, corps, alors, aurore. We hear it in one of the several mispronunciations of board, *oar*, torn, forth, &c.

IV. THE *u* VOWELS.—The palato-lingual passage reaches another step forward on the tongue, and to a higher point upon the soft-palate; the vocal current is nearly vertical in direction, and the soft-palate is arched upward extremely: the group stands as the terminus of an ascending series from the throat.

In this group, the tongue, in passing from open to close, is perceived to be distinctly elevated as well as retracted.

Degree 1.—*Vowel* u^{1l}. The closer and usually longer *oo*, as food, &c.; the *o* in do, &c.; *oe* in shoe, &c.; *ou* in you, &c.; the main and final part of the compound in union, view, few, beauty, &c. Whether the *u* in rude, ruin, &c., should take a slight initial sound of another vowel, is made a matter of question. I think it strikes, or should do so, a more open vowel of the group, and then falls upon this; as it does also in dew, new, tube, lure, suit, &c.

This is the long *u* vowel of most European languages. In French, it is the long *ou*; the letter *u*, as alone, having early gone over from this to a vowel group further forward.

The labial contraction is closer in this vowel than in the close *o*. The vowel has a peculiar mellow smoothness, and imparts the same character to the *o*, when added thereto as a vanish. It is possible to utter the palato-lingual part of this vowel without the labial modification, but not smoothly and with perfectly pure vowel quality.

Degree 2.—*Vowel* u^{2l}. F*u*ll, p*u*sh, b*o*som, sh*ou*ld, g*oo*d, f*oo*t, &c. In the proper pronunciation of these words, the lips are contracted to nearly or quite the same degree as for the close *o* simple. It is the final element in the diphthong *our*, n*ow*, r*ou*nd, and the usual vanish of w*oe*, l*ow*, r*o*ll, &c., the long *o*. In French, we have it in c*ou*p, b*ou*t, b*ou*rse, &c., that is, the shorter *ou*. It is the short or middle *u* of the German and of most of the languages of Europe.

Vowel u^2. This non-labial is frequently used in America, improperly, in place of each of the two preceding, as in f*oo*t, s*oo*t, r*oo*t, r*oo*f, s*oo*n, b*oo*k, sh*oo*t, f*u*ll, p*u*t. If I mistake not, it is the proper form of the shortest *u* in German, as in d*u*rch, &c.

Degree 3.—*Vowel* u^3. The unaccented f*u*lfill, willf*u*l, &c.; also, t*o*, d*o*, &c., when unemphatic and somewhat slurred; also formerly common and still to be heard in New England in some, if not all, of the class of words just specified, f*oo*t, s*oo*n, &c. Young misses who mince their words will pronounce t*wo*, as well as t*oo*, in this way. But slight change in the action of the organs is needed to convert this utterance into either a short *i* or a French *u*.

Degree 4.—*Vowel* u^4. Heard, as I incline to think, in the Scotch g*u*de or g*ui*d, s*u*ne, s*u*ld, bl*u*de or bl*ui*d, d*u*re, &c. (for *good*, *soon*, *should*, *blood*, *door*, &c.), with perhaps a vanish in another (an *i*) vowel. Occurs in no case in well-spoken English, except as the initial element in the best pronunciation of d*ew*, n*ew*, t*u*be, l*u*te, &c. (Princ. of Pron., § 30.)

V. THE *ö* VOWELS.—The palato-lingual tube reaches yet further forward, but hardly beyond the extreme fore-part of the soft palate. The edges of the tongue join the borders of the palate about as far as to the hinder teeth, and the tip of the tongue is naturally further forward than in the more close of the degrees in the preceding group.

In this and in all the remaining groups, the degrees of close and open are made by the greater and less elevation or depression of the tongue, and not at all by its retraction.

Degree 1.—*Vowel* o^{1l}. A sound strange to English ears; the long *ö* in German, as sch*ö*n, K*ö*nig; the long close *eu* in French, as je*û*ne, heur*eu*se, f*eu*x.

Degree 2.—*Vowel* $ö^{2l}$. The shorter German *ö*, as Wörter, möchte; the French *eu* in l*eu*r, j*eu*ne, p*eu*r, &c.

Vowel $ö^2$. There is, in English, a class of cases in which *e*, i, *y*, or *ea*, followed by *r*, takes properly this articulation; as err, m*e*rcy, v*i*rtue, b*i*rd, g*i*rl, m*y*rrh, m*y*rtle, *ea*rl, p*ea*rl, *ea*rth, earn. It is very common, indeed, to give here the sound of *u* n *u*p, b*u*t, or b*u*rn, *u*rge. But orthoëpists agree, for the most part, that a different utterance in these cases is sanctioned by he best usage,—without being well agreed, however, as to its precise character. As I here describe it, the vowel differs from he French l*eu*r simply in the absence of the labial modification. See Prin. of Pron., §§ 14, 18.)

Degree 3.—*Vowel* $ö^3$. The *u* in *u*p, b*u*t, &c.; *o* and *oo* in d*o*ne, *o*mpany, fl*oo*d, &c.; *oe* in d*oe*s;—unaccented syllables tend to his sound, as alt*a*r, off*e*r, tap*i*r, zeph*y*r, verb*a*l, bedl*a*m, ball*a*d, method, &c. Frequent in English, but rare in other European ongues. In French, occurs nasalized in *u*n, br*u*n, &c., and is he so-called "*e* feminine," as in d*e*, c*e*, demand*e*, dam*e*, when not elided,—unless this takes somewhat of a labial modification.[9]

This English vowel is described by some German grammarians as approaching the short German *ö* and lying between that and the short *o*.[10] By some English phonologists, it has been called "the natural vowel," and by others "the neutral vowel"; and for the most part they seem at a loss how to locate it in their systems. Dr. Rapp, in his *Physiologie der Sprache*, styles it the *Urlaut*, *Urvocal*, the original, or primitive vowel. It is by some described as "the unmodified vowel." To perceive that this vowel is not mere vocal tone unmodified, we need but to notice the fixed, rigid position of the tongue in the utterance. The vocal element, or tone, in the consonants *v*, *z*, &c., is unmodified vowel-wise, but is clearly unlike the vowel in question. Only, when the vowel is slurred and almost elided, as in p*e*rform, tok*e*n, or Fr. s*e*rez, d*e*là, there is probably no determinate vowel modification.

Degree 4.—*Vowel* $ö^{4l}$. The best French orthoëpists have distinguished the sound of *eu* before *r*, in b*eu*rre, c*oeu*r, &c., as a broad and open one; as do also some of the best instructors. It is recognized by the ear as an approach to an open-depressed *a* or *å*, and falls exactly into the place here assigned it.

Vowel $ö^4$. This, as by itself, we have only to note as a broad, flat, drawling utterance of the *u* in *u*p, b*u*t, ($ö^3$), and by some clerical speakers affected in ch*u*rch, w*o*rk, &c.

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[9] Prof. L. Vaïsse ranks this *e* as a "labio-palatal." Palsgrave (1530) ascribes to it a nasal quality: says the reader should "sodeynly depresse his voyce when he cometh to the soundynge of hym, and also sounde hym very moche in the noose." *L'Eclaircissement, &c.*: ed. Genin.

[10] See Mätzner, Eng. Grammatik, i, 14; and Fiedler, p. 115.

This element is, however, important as the initial of two diphthongs, viz., *ou* or *ow* (*ou*r, n*ow*), and long *i* (*i*ce, k*i*te), according to the best usage. The Scotch give the long *i* as $\ddot{o}^{2}+i^{1}$; some of the North of England dialects as $\dot{a}+i$; we sometimes hear it as $a^{2}+i^{2}$, sometimes as $\ddot{a}^{2}+i^{2}$, not to speak of other variations.

VI. THE *ä* VOWELS.—The vowel-tube, reaching a step further, fairly laps upon the hard palate above; and the borders of the tongue will there meet either the teeth or the gums or palate on each side. The tip of the tongue will be naturally more advanced than for the preceding group.

Degree 1.—*Vowel* $\ddot{a}^{1}$. The longer German *ä*, as M*ä*dchen, w*ä*re, &c. The long and grave German *e*, as l*e*ben, g*e*ben, gel*e*gen, is the same, unless somewhat less close.[11] Also, the "open-grave" *è*, so-called, in French, as apr*è*s, sc*è*ne, jam*ai*s, f*ai*re, p*è*re, &c. In other languages of Europe, also, *e* sometimes takes a similar sound, improperly described as "open." Thus in Spanish, as we are taught in Sales' Grammar, "before *n*, *r*, *s*, *z*, in the same syllable, *e* is pronounced more open, as in the English words c*a*re, sn*a*re." In Italian, *e* has such a so-called open sound in numerous words.

In English, this vowel occurs only as followed by *r*. H*ei*r, th*ei*r, f*ai*r, f*ai*ry, p*a*rent, p*ai*r, b*ea*r, &c., take properly either this, or else the middle degree ($\ddot{a}^{2}$), usage being diverse; and many give improperly the "long *a*" sound (e^{1}). I incline to a discrimination, and would give to h*ei*r, th*ei*r, p*a*rent, and some others, the close degree, and the middle to th*e*re, wh*e*re, h*ai*r, pr*ay*er, c*a*re, p*ea*r, and others. The influence of some professed orthoëpists, whose obtuseness has led them to ignore the distinction between this vowel and the "long *a*," has tended to expel the sound from the language. Still, as respects the English "long *a*" itself, the usage is less settled and uniform than is generally supposed. In some quarters it even takes the vowel sound here in question, which once regularly belonged to it,[12] and this cause may have helped to obscure the distinction just alluded to. Such a use is marked by Alex. M. Bell, of Edinburgh, as "an oratorical and especially a pulpit Scotticism," as "in *nation*, *education*, *gracious*, &c." (Elocutionary Manual, &c., Edinb., 1859.) But, as I am told that it is a marked characteristic in the pronunciation of some English public speakers, the Rev. James Martineau among others, and as I have heard it not unfrequently from native Americans, I am disposed to regard it rather as a relic of olden time which traditional habit has preserved to some extent, and more especially in the pulpit and on the stage.

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[11] The distinction between the *e* in l*e*ben, &c., and the proper long *e* (see the *e* group), is noted by Jacob Grimm and others, but would seem not to be universally observed at the present day. Heyse, in his *Schulgrammatik*, describes this *e* as similar to the *ä*. Possibly it would come nearer to our e^{4}, which is somewhat liable to be confounded with $\ddot{a}^{1}$.

[12] Originally, the *a* in English had a proper *a* sound, and it was only by gradual change that the "long *a*" of the present time came about. Two hundred years ago, Dr. John Wallis and Bishop Wilkins knew no other English sound of *a* than what they both describe expressly as the Italian sound, long and short. The *ai*, which now almost invariably takes the "long *a*" sound,—p*ai*l, m*ai*n, not differing from p*a*le, m*a*ne,—was then usually a proper diphthong, the same now used in the word *aye*, and sometimes heard yet in Is*ai*ah, Sin*ai*, *ai*sle. In the gradual change from the original to the present sound, the vowel-group we now treat of would naturally be taken on the way; and, in the case of the diphthong, probably the initial element first came into this group, and then, dropping the final and taking a

It is not unusual to describe the vowel in question as identical with the *e* in *met* prolonged. The truth is that to prolong the *e* in *met* without change of quality is difficult, and the attempt is apt to produce this vowel as a matter of fact; yet the two are really different in vowel quality. The description is good as a practical rule, but not to be accepted as a true analysis.

Degree 2.—*Vowel* $\ddot{a}^2$. In treating of the vowel above, we have remarked sufficiently upon this, as concerns the English. In German, the shorter *ä* takes this sound in many words, as *rächen*, *dämmern*, though in perhaps the greater number it does not differ essentially from the short *e*; but there is a want of uniformity in its pronunciation by the Germans themselves.

Degree 3.—*Vowel* $\ddot{a}^3$. The so-called short *a* in English, as *at*, *cat*, *man*. Not heard in German. In French, modern usage inclines somewhat to substitute this for the more proper close *a*.

Degree 4.—*Vowel* $\ddot{a}^4$. This is heard in the drawling utterance of the short *a* (*cat*) in the genuine Yankee dialect, which also substitutes it for $\ddot{a}^1$ or $\ddot{a}^2$, as in wh*ere*, h*air*, &c., and, besides using it conspicuously in *are*, makes it the initial element in *our*, *cow*, n*ow*, r*ou*nd, the whole compound being nearly $\ddot{a}^4+\ddot{o}^3+u^3$. The vowel involves such an action of the *velum palati* as superadds a decided nasal quality. Fully nasalized, it is the French *in*, as v*in*, f*in*, cous*in*, &c.

close form, passed over to the present "long *a*," which lies in the next group forward. Examples of the transitional usage were observed,—within the present century, of course,—by the eminent phonologist and linguist, Dr. Erasmus Rask, to such extent as to give him the idea that this was the usual pronunciation of the English long *a* and *ai*. In his *Danish Grammar for the use of Englishmen* (ed. Repp), he notes two sounds of Danish *e*, as like the "French *é fermé* and *è ouvert*," and says: "The *é* ferme, or close *e*"—which we have yet to describe—"is very frequent in Danish, but not of frequent occurrence in English; still it is found in such words as *their*, *vein*, which have a different sound from *there*, *vain*,"—these, of course, being meant as like the "open *e*." He had also just said: "The open *e* is exactly like the *æ*, but usually short," and had described the *æ* as equivalent, long and short, to the English *a* in *sale* and *ai* in *said*, respectively.

I do not forget that Dr. Rask is regarded by some as more nice than wise in these matters; but somewhat unjustly, I suspect. In the second edition of his Danish Grammar, edited by Repp, and professedly a faithful reproduction of the first, we find no such distinction made between *pale* and *pail* as Mr. Marsh ascribes to him. (See *Lectures on the English Language*, by G. P. Marsh, first series, p. 285.) Did Mr. M.'s usual accuracy fail him in this case?

VII. THE *e* VOWELS.—The passage is extended still further on the hard-palate and the tongue, and the tip of the tongue will naturally be found further forward.

Degree 1.—*Vowel* e^1. The "long *a*" in English, as in *a*le, f*a*te, gr*e*at, v*e*in, h*a*il, d*ay*, &c.; also, the more usual German long *e*, as in m*e*hr, j*e*der; the other, in l*e*ben, &c., has been already described as $\ddot{a}^1$. Possibly, some one of the two or three or more varieties of the "open *e*" in French may not differ materially from this.

This vowel, in English, more commonly takes a vanish in an *i* vowel (p*i*que, p*i*n, &c.); as it plainly does always in s*ay*, r*ay*, h*a*il, &c.

Degree 2.—*Vowel* e^2. Occurs in English in lightly accented or unaccented syllables; as nitr*a*te, carbon*a*te, clim*a*te, parl*ia*ment, lapid*a*ry, compar*a*tive, &c. In such a case as *e*dge, the consonant inclines the vowel to this instead of the open sound of *e* in get, though *e*dge would never be made close like *a*ge. The long *a* is usually struck upon this degree, falling quickly upon the closer sound for the main part, and ending off with the vanish in *i*; e. g., n*a*me, c*a*me, p*a*in; but in quick utterance, the closer sound is not given at all.

This is the usual shorter *e* of the German, accented and unaccented, as f*e*rtig, K*e*ller, Lieb*e*, Vat*e*r.

Degree 3.—*Vowel* e^3. The English short *e*, so-called, as in g*e*t, *e*gg, r*e*d. We do not hear precisely this in the shortest French *e*, c*e*tte, tromp*e*tte, nor in the German, as d*e*nn, B*e*tt, &c.;—*denn* is not just the English *den*, nor is *sechs* the same with *sex*.

Degree 4.—*Vowel* e^4. To this I am disposed to assign the French *ê*, as in t*ê*te, f*ê*te. In English, the long *a* may be sometimes drawled into this form, and singers do this sometimes with the short *e*, as in s*e*lf, t*e*n, for example; but in each case it is a flat and faulty pronunciation.

VIII. THE *é* VOWELS.—The passage reaches well on to the forward part of the hard-palate and the tongue.

Degree 1.—*Vowel* $\acute{e}^1$. The close *é* and *ai* of the French, as bont*é*, cit*é*, j'*ai*, aimer*ai*. The sound may sometimes be given to the English long *a*, but such is not the usual pronunciation.

Degree 2.—*Vowel* $\acute{e}^2$. The French "open acute" *e* and *ai*, as c*e*tte, ci*e*l, *ai*mer, m*ai*son; also heard in certain unaccented syllables in English, as guin*ea*, vall*ey*, carr*ie*d, coll*e*ge, r*e*sist, pr*e*pare, app*e*tite, lev*e*l, bus*y*, cit*y*, and in the vulgar *a* final, as Americ*a*, Cub*a*, Eliz*a*. The vowel is somewhat difficult to discriminate from e^2, in the preceding group.

Degree 3.—*Vowel* $\acute{e}^3$. To this belongs, I think, the German denn, Bett, &c., and the shortest French *e*, as trompette, &c. It occurs, unaccented, in goodn*e*ss, knowl*e*dge, trump*e*t, &c.,—the right sound here being not the short *i*, nor the regular short *e*, but intermediate.

Degree 4.—*Vowel* é^4. The Swedish long é, as Carlén, from the best information I have, would appear to be correctly described s this vowel. Dr. Thomas's description of it (Webster's Dicionary, new ed., p. 1634) as "a sound resembling that of short prolonged," would bring it very near to this.

IX. The *i* Vowels.—The most advanced group in the scale of palato-lingual position. The borders of the tongue are applied to the palate, not indeed clear to the tip as for the consonant *s*, but as far forward as can be, seeing the place of articulaion is a vowel-tube reaching from the throat, instead of simply a point as it is for the consonant. It is also to be observed that he middle and back part of the tongue is raised to a position omewhat like that for the *u* vowels, and with a similar, or even greater, arching up of the soft-palate.

I give this as the precise arrangement which brings out the sound most distinctly and most naturally. But, in all the anterior groups, as before remarked, owing to the extensile structure of the tongue, the articulation may have a determinate and nearly invariable place upon the palate, and yet reach to a variable point on the tongue. Thus, in this case, the tongue may be thrust forward, with the tip and fore part depressed behind the lower teeth; the terminus of the vowel-tube falling further back, of course.[8] The variation is the same as may occur in the so-called dental consonants, *t*, *n*, *d*, which are properly made with the tip of the tongue, but can be uttered by using a part of the tongue considerably further back.

The peculiar shape of the palatal arch, as it converges forward and gives to the passage a rounded form, would seem to bear an essential part in producing the vowels of this group. If they can be imperfectly imitated at a place further back on the palate, it is done only by so shaping the tongue as to make a somewhat similar converging and rounded passage.

Degree 1.—*Vowel* i^1. Mach*i*ne, f*ie*ld, *ea*t, &c., and the long *e*, and *ee*, as *e*ve, m*e*ter, d*ee*p, &c.; the long *i* on the continent, as (Fr.) av*i*s, l*i*re, am*i*e, and (Ger.) M*i*ne, m*i*r, w*i*der.

Vowel i^{1l}. The long *u* of the French, as r*u*se, gr*u*e, and long *ü* of the German, as *ü*ber, Sch*ü*ler. As commonly uttered, it might, if without the labial modification, form a somewhat impure *i*, made such by some admixture of a consonantal *y*. This vowel probably nowhere exists as developed from an original *u* vowel.

Degree 2.—*Vowel* i^2. The so-called short *i* of the French, as am*i*, f*i*dèle, v*i*f, and of the German, as m*i*t, b*i*tten, n*i*cht. In English, may be heard in v*e*hement, veh*i*cle, d*i*vine, mit*i*gate,

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[8] So represented in the diagram in Max Müller's Lectures, second series, p. 134, which is also more seriously at fault in placing the point of approximation quite too far back upon the palate.

&c.: and, in mandar*i*n, capuch*i*n, chlor*i*d, &c., is preferable to the closer sound. It makes the usual vanish of "long *a*" in n*a*me, pr*ai*se, &c., the whole being commonly $e^2+e^1+i^2$; also, the final element of "long *i*," *i*ce ($ö^4+ö^3+i^2$), and of *oi*, *oy*, as to*i*l, bo*y* ($\mathring{a}^{2l}+i^3$).

Vowel i^{2l}. The shorter French *u*, as *u*ne, r*u*de, r*u*ban, perhaps a little more open in b*u*tte, r*u*sse, &c.; and the shorter German *ü*. as Gl*ü*ck, w*ü*nschen, M*ü*tter. We have in this—and not in the simple i^2 or i^1—the initial part of the English long *u*, as *u*nion, *u*se, t*u*be, m*u*te, and of *you* in *you*th, *you*, &c., and *eau* in b*eau*ty. Between this and the main and final element, there is a distinct consonantal *y*. So that the long *u* is $i^{2l}+y+u^{1l}$. I propound this,—with deference of course to the "Autocrat,"—as the secret of how to pronounce the word *view*.

Degree 3.—*Vowel* i^3. The "short *i*" in p*i*n, h*i*t, g*i*ve, &c. The shortest French *i* may sometimes approach this, as in pet*i*te, r*i*sque, v*i*lle, and the German in b*i*tten, *i*st; but are hardly, we think, to be ranked here.

Degree 4.—*Vowel* i^4. Heard in an improper prolongation of our short *i* (i^3), sometimes as a faulty general habit, and sometimes used in the way of emphasis; properly, the short vowels admit only the emphasis of force or stress, and the syllable should be prolonged, if it all, only on the consonant. The rustic New England d*o*, r*u*de, sm*oo*th, &c., sometimes takes this for the initial element.

Having thus completed the analysis of the simple vowel elements, I will suggest an experiment by which I am willing to abide as *a test of the correctness of my theory*. It is easy to observe, in the first place, that i^3 (*i*t, k*i*n) is more open than i^1 (*e*at, k*ee*n); or e^3 (*e*ll, p*e*n) than e^1 (*a*le, p*a*ne); and, in general, that the so-called short are more open than the corresponding long under the several groups,—which, indeed, has not been unrecognized as a fact by orthoëpists (as see Princ. of Pron., Notes to §§ 2, 8, 11, 16, 25, 31). This point being settled, if then we articulate the series of close vowels in order from front to rear, viz., i^1 (*e*ve), $é^1$ (aim*é*), e^1 (f*a*te), $ä^1$ (th*ei*r, M*ä*dchen), $ö^{1l}$ (K*ö*nig, je*û*ne), u^{1l} (*oo*ze), o^{1l} (*o*ak), $\mathring{a}^{1l}$ (*aw*e), a^1 (p*a*st), we shall find the position of the organs such that, if we suppose a wedge inserted between tongue and palate, it would enter further and further with each successive vowel, being stopped at each point by the front terminus of the palato-lingual passage. We have only to use a thin rod, or even a finger, to perceive this. Observe that, at the point of the wedge supposed inserted, there will be constantly found a close position: showing that the vowel station is simply carried back, and that the difference is not one of merely open and close. The same thing may be done with the series of open vowels i^3, $\acute{e}^3$, &c. (p*i*n, Ger. d*e*nn, *e*nd, c*a*t, b*u*t, willf*u*l, n*o*t, n*o*r, Fr. b*a*s or Eng. b*a*lm),—and the same fact of the regress of the vowel station will be observed. Experiments of this sort, fairly made, seem to me to furnish complete demonstration of the leading principles on which I insist. Further proof will appear in the sequel.

If, on the other hand, we try to arrange all the vowels in a single series on any principle whatever, we find ourselves utterly baffled. If we distinguish them as simply more and less open or close, we find confusion instead of order. Nor should these terms *open* and *close* be applied otherwise than as I have done. It is true they might not unaptly be used to describe the difference as the vowel station moves back toward the throat or forward from it,—even as they might describe the corolla of a flower unfolded down toward the base or only near the tip,—but the terms are wanted to indicate the width of the expansion, and must therefore not be used for the depth. Neither is the confusion escaped by setting the labials in a class by themselves, arranged according to the extent of the labial opening: the labials cannot be all so discriminated, if we include all in actual use, while for the non-labials the difficulty still holds. Nor will any other subdivision answer, which falls short of the groupings, or substantially such, as in the scheme here presented.

[To be continued.]

Art. XXVI.—*On Photo-micrography with the highest powers, as practised in the Army Medical Museum*; by J. J. Woodward, M.D., Asst. Surgeon and Brevet Major U. S. Army, in charge of the Record and Pension Division Surgeon General's Office, and of the Medical Section Army Medical Museum.

Photography had but just begun to attract attention when the attempt was made by Donné to reproduce microscopic objects by the Daguerrean process; and although the results of these experiments were far from satisfactory, they promised enough to lead to further efforts in this direction, renewed with each step in the gradual improvement of the photographic art. These exertions were crowned by a continual progress, which did not however keep pace with the development of other branches of photography, though it must be admitted that in the hands of the more modern experimenters, and especially of Prof. Gerlach of Erlangen, Jos. Albert of Munich and Dr. R. L. Maddox of Southampton, the success has been such as to guarantee a wide field of usefulness for this method of representation.

In America, the chief experimenters have been Prof. O. N. Rood of Columbia College and Mr. Lewis M. Rutherfurd of New

York. Besides these, mention must be made of the paper of Dr. John Dean of Boston on the Spinal Cord, which is illustrated by photomicrographs reproduced by photolithography. The work of Dr. Dean however was done with magnifying powers not exceeding ten or twelve diameters, while both Professor Rood and Mr. Rutherfurd have experimented with very high powers.

Prof. Rood published a very interesting account of his process in this Journal in 1861.[1] Omitting details, it appears from this paper that in his operations, he used direct sunlight for illumination, and employed ordinary achromatic objectives with or without eye-pieces. The difference between the visual and chemical foci he endeavored to overcome by an alteration of the fine adjustment after the plan suggested by Shadbolt.[2] Prof. Rood thus obtained photographs, chiefly of diatoms, so far as I have been able to learn, with powers as high as the $\frac{1}{7}$th objective, which gave with five feet distance 460 diameters, with about three feet distance and the long eye-piece 1300 diameters. The pictures thus obtained compared favorably with any which have been taken with achromatic objectives. In May, 1865, Mr. Lewis M. Rutherfurd, of New York, published a paper on Astronomical Photography,[3] which contained the following suggestive passages. "The image of a star at the focus of a perfectly corrected objective would be a point, the apex of all conceivable cones having the object glass or parts of it as the bases. This point falling upon a prism would be converted in a line, red at one end and violet at the other, with the intermediate colors in their proper places. If, however, the different colored rays are not all brought to the same focus, the spectrum will no longer be a line, but in the uncorrected colors will be expanded to a brush the width of which will be the diameter of the cone where intercepted by the prism. It will thus be seen that a simple glance at a star spectrum will indicate at once what parts of the spectrum are bounded by parallel lines and consequently converged to one focal point, and what parts do not conform to this condition, and also the amount of divergence. On applying this test I found that an objective of flint and crown in which the visual was united with the photographic focus, (in other words, where the instrument could be focalized on a plate of ground glass by the eye, as in ordinary cameras, and in the heliographs constructed by Dalmayer for the Kew observatory and for the Russian government,) is a mere compromise to convenience in which both visual and actinic qualities are sacrificed."

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[1] On the practical application of Photography to the microscope; by Prof. O. N. Rood; vol. xxxii, p. 186.

[2] On the photographic delineation of microscopic objects by artificial illumination. By Geo. Shadbolt, Esq. Quarterly Journ. of Microscopical Science, vol. i, p. 165.

[3] Astronomical Photography; by Lewis M. Rutherfurd, this Journal, xxxix, 304.

"In order to bring the actinic portion of the spectrum between parallel borders, i. e., to one focus, it is necessary that a given crown lens should be combined with a flint which will produce a combined focal length about one-tenth shorter than would be required to satisfy the conditions of achromatism for the eye, and in this condition the objective is entirely worthless for vision." With a telescopic objective constructed on this principle, Mr. Rutherfurd obtained telescopic photographs of such satisfactory quality that he concludes his paper as follows:

"The success of this telescopic objective has encouraged me to hope that an almost equal improvement may be made for photography in the microscope, which instrument is more favorably situated for definition than the telescope, since it is independent of atmospheric conditions. Its achromatic status is easily examined by the spectroscope, using as a star the solar image reflected from a minute globule of mercury. Mr. Wales is now constructing for me a one-tenth objective, which, upon his new plan, is to be provided with a tube so arranged as to admit the removal of the rear combination, and in place of the one ordinarily used, one is to be substituted at will which shall bring to one focus the actinic rays."

This objective was satisfactorily constructed by Mr. Wales (of Fort Lee, N. J.) and Mr. Rutherfurd made with it a number of experiments, full of promise, though his other pursuits prevented him from following out the new plan to its ultimate results.

Such was the condition of photo-micrography in America when it occurred to me to resort to this method of illustration in preparing proper representations of the histological studies of camp diseases which have been made by me or under my direction for the Official Medical History of the War of the Rebellion.

I at once visited Mr. Rutherfurd, whose paper had attracted my attention, and I received from him many important suggestions which I desire to acknowledge in the fullest manner. Among these I may especially mention the plan of constructing the objective above indicated, the use of the ammonio-sulphate of copper, and the suggestion of substituting a properly constructed concave for the eye-piece.

In developing these suggestions the actual manipulations were assigned to Asst.Surgeon and Brevet Capt. Edward Curtis, U.S.A., of the Army Medical Museum, and to his patience, tact and ingenuity I am indebted for the successful issue of the experiments which were undertaken. The results attained have been most satisfactory, excelling, as is confidently believed, anything heretofore done in this direction. This is also the opinion of Dr. Maddox, whose judgment is of the greater value as he is one of the most successful laborers in this direction in Europe.

The principles involved in obtaining successful photographs with the microscope are the following:

1. To use objectives so corrected as to bring the actinic ray to a focus.
2. To illuminate by direct sunlight passed through a solution of ammonio-sulphate of copper, which excludes practically all but the actinic extremity of the spectrum.
3. Where it is desired to increase the power of any objective, to use a properly constructed achromatic concave instead of an eye-piece.
4. To focus on plate glass with a focusing glass, instead of ground glass.
5. With high powers to use a heliostat to preserve steady illumination.
6. Where an object exhibits interference phenomena when illuminated with parallel rays, as is the case with certain diatoms and many of the soft tissues, to produce a proper diffusion of the rays by interposition of one or more plates of ground glass in the illuminating pencil.

Strict adherence to these principles is indispensable to success. In the Museum they have been carried out by the following details:

A camera is not used, a dark room being found most convenient. The operating room has two windows, through one of which just enough yellow light is admitted to permit the movements of the operator. The lower part of the other window is occupied by a shutter about fourteen inches high on which the blackened sash shuts down light-tight. In this shutter is a round hole an inch and a half in diameter, from the inner side of which a brass tube of the same diameter projects into the room. On the outer side of the hole is a rod about twelve inches long, on the extremity of which the microscope mirror is duly centered. Two steel rods attached by hooks to the mirror and passed through the shutter permit its position to be adjusted by a person standing inside of the room, without opening the window. A Silbermann's heliostat standing on a shelf just outside of the window throws the sunlight steadily upon the mirror. Within the room a frame of walnut, ten feet long, is placed on a firm table perpendicular to the window. The microscope stands on the end of this frame next the window, its mirror is removed, being replaced by that outside the shutter. The microscope is placed in a horizontal position, and the tube carrying the diaphragm or the achromatic condenser fits into the tube projecting inward from the shutter by which the sun's light reflected from the mirror outside is admitted. A black velvet hood covers the parts about the stage and objective of the microscope, and thus prevents the leakage of light into the room.

The plate holder is movable backward and forward on the walnut frame on which the microscope stands, its maximum distance from the stage of the microscope being nearly nine feet.

To permit ready focusing at distances greater than the length of the arm, a wooden rod $\frac{3}{4}$ths of an inch in diameter and capable of easy rotation runs the whole length of the right side of the frame. The milled head of the fine adjustment of the microscope is grooved, and a small grooved wheel in the end of the rod permits the two to be connected with a band. The operator standing at any part of the frame can therefore manipulate the fine adjustment by simply turning the wooden rod in his fingers.

The arrangements of light, position of object, coarse adjustment, &c., are made by the operator, who stands by the microscope, which has a suitable eye-piece adjusted, and observes the object in the usual way; afterwards, removing the eye-piece and going to the plate holder, the final focusing is made by means of the wooden rod, the image being viewed with a focusing glass on a piece of plate glass held in the same frame which is to receive the sensitive plate.

The cell containing the ammonio-sulphate of copper hangs outside the shutter over the hole by which light is admitted. It not only excludes the unnecessary illuminating rays, but prevents danger to the objective from the concentrated solar heat and permits the eye of the operator to view the objects about to be copied without fatigue or injury. Latterly a plate of alum has also been used to exclude solar heat especially during any temporary removal of the ammonio-sulphate cell. The chemical processes employed are well known to all photographers. With the above apparatus, it has been found that the best defined pictures are obtained when the distance employed with any objective does not exceed three or four feet.

The achromatic concave used as a substitute for the eye-piece is a combination of somewhat more than half an inch transverse diameter, and about 28° angle, constructed, like the objective, to focus the chemical rays. It increases the magnifying powers of the objective about seven times. It has been found to perform well with both the $\frac{1}{8}$th and $\frac{4}{16}$th.

In photographing the soft tissues or other objects in which illumination with parallel rays produces interference lines, the ground glass is to be placed between the mirror and condenser. Of course, there is considerable diminution of light, but this can be overcome, for the higher powers, by condensing the sun's light on the ground glass by a bulls-eye or other similar contrivance. If the interference lines as seen by the eye do not disappear with one thickness of ground glass, two or more may be used.

The most powerful objective with which photographs have been taken in the Army Medical Museum is a $\frac{1}{50}$th, manufactured recently for the Museum by Messrs. Powell and Lealand of London. The subject selected for the experiment was *Pleurosigma angulatum.* With the $\frac{1}{50}$th and three feet nine inches distance and without an eye-piece, a picture of a portion of a frustule was obtained magnified 2,344 diameters. This negative readily bore enlargement to 19,050 diameters. The field in the picture is six inches in diameter and is remarkably sharp in the center, but shows considerable curvature and on the edges is quite out of focus. Further experiments with the $\frac{1}{50}$th satisfied us that a greater power could not be advantageously obtained from it.

About the same time experiments were made with the Wales' $\frac{1}{8}$th, due amplification being given by the achromatic concave. It was intended to obtain with this the same power as with the $\frac{1}{50}$th, but, although the distance was reduced to 3 feet, the subsequent measurements showed 2,540 diameters, or about 200 diameters more than were obtained with the $\frac{1}{50}$th. This was the maximum performance of the $\frac{1}{8}$th and readily bore amplification to 19,050 diameters. The field thus obtained with the $\frac{1}{8}$th, over seven inches in diameter, was absolutely flat. I send you herewith albumen prints of both sets of pictures. You will observe that the small pictures with the $\frac{1}{50}$th are the sharpest, owing in our opinion to somewhat better chemistry in making the negative, while, of the enlargements, that from the $\frac{1}{8}$th picture is best, owing to the greater flatness of the field in the original negative.

Without going into a discussion of the comparative merits of Powell and Lealand's $\frac{1}{50}$th in this place, it is interesting to observe that these photographs confirm the opinion expressed by Prof. Rood in this Journal[4] as to the circular nature of the markings on *Pleurosigma angulatum*, an opinion which had previously been expressed by Mr. Wenham.

At the date of publication of Circular No. 6, Surgeon General's Office, both Dr. Curtis and myself believed these markings to be hexagonal, as was stated and figured on page 148 of that work. The greater power now obtained has corrected this opinion, but it is worthy of note that in the present pictures the markings appear hexagonal in both the small ones, if viewed with the eye at the visual distance, while on close inspection or with a lens they are seen to be circular. In the pictures with 19,050 diameters the circular shape of the markings is very plain, but if viewed from a considerable distance or with a concave lens, they appear hexagonal. I also send you herewith a photograph of cartilage magnified 370 diameters, in illustration of the results attainable in the photography of the soft tissues. This picture shows capsules, corpuscles and nuclei with the utmost sharpness.

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[4] On the evidence furnished by Photography as to the nature of the markings on the Pleurosigma angulatum; by Prof. O. N. Rood, this Journal, vol. xxxii, p. 335.

In short, it is our opinion that henceforward photography is indispensable to the proper representation of microscopic objects, and is, as practised in the Army Medical Museum, even in its present condition, adequate to the satisfactory representation of all microscopic objects that do not depend for their value on colors.

ART. XXVII.—*Note on a Regular Dimerous Flower of Cypripedium candidum;* by ASA GRAY.

Mr. J. A. Paine, Jr., of New York, who two years ago detected an interesting monstrosity of *Pogonia ophioglossoides*, has now brought to me, preserved in spirit, a monstrous blossom of *Cypripedium candidum*, which demands a record.

The plant bears two flowers: the axillary one is normal; the terminal one exhibits the following peculiarities. The lower part of the bract forms a sheath which encloses the ovary. The labellum is wanting; and there are two sterile stamens, the supernumerary one being opposite the other, i. e., on the side of the style where the labellum belongs. Accordingly the first impression would be that the labellum is here transformed into a sterile stamen. The latter, however, agrees with the normal sterile stamen in its insertion as well as in shape, being equally adnate to the base of the style. Moreover the anteposed sepal is exactly like the other, has a good midrib and an entire point. As the two sterile stamens are anteposed to the two sepals, so are the two fertile stamens to the two petals, and the latter are adnate to the style a little higher than the former. The style is longer than usual, is straight and erect; the broad, disciform stigma therefore faces upwards; it is oval and symmetrical, and a light groove across its middle shows it to be dimerous. The placentæ, accordingly, are only two. The groove on the stigma and the placentæ are in line with the fertile stamens.

Here, therefore, is a symmetrical and complete, regular, but dimerous orchideous flower, the first verticil of stamens not antheriferous, the second antheriferous, the carpels alternate with these; and here we have clear (and perhaps the first direct) demonstration that the orchideous type of flower has two stamineal verticils, as Brown always insisted.

ART. XXVIII.—*Contributions from the Sheffield Laboratory of Yale College.*—XII. *Analysis of a Mineral Water;* by FREDERICK F. THOMAS, Ph.B.

THE mineral spring, the water of which is the subject of this notice, is situated in the town of Barton, Tioga Co., New York, about seven miles northeast of the village of Waverly, near what is called Talmadge Hill. It is one of two sulphur springs that have been observed in that county. The other, resembling it in character, is about twenty miles north, near the village of Spencer; both have been noticed in the State geological reports, and have been in repute for many years among the inhabitants of that region on account of certain remedial properties which their waters are supposed to possess.

This spring rises from rocks of the Devonian age—the blue argillaceous shale and sandstone of the Chemung group—of which many outcroppings may be seen in the immediate neighborhood of the spring. These rocks, according to examinations made in this Laboratory, consist principally of silicate of alumina, but also contain sulphuric acid, lime and magnesia in considerable quantities, some potash, soda and iron, as well as chlorine, organic matter, and a trace of manganese. They turn black upon heating before the blowpipe, but no effervescence is observed on treating them, in the pulverized state, with acids. The water partakes considerably in its mineral character of the properties of the rocks at the surface, but contains a very small amount of sulphates, while a large percentage of its mineral constituents are carbonates. The mean of two corresponding determinations of sulphuric acid made on the water as soon as received at the laboratory, gave 0·116 grain per gallon, while other estimations, made from water which had remained sealed in bottles for some weeks, gave a slightly larger amount, which resulted from oxydation of sulphur.

As the rocks in which the spring rises contain traces of manganese, that substance would probably be found in the water, were a large amount concentrated. None was however detected in the quantity of water at disposal.

The sulphur and sulphuretted hydrogen in the water may be due to the reduction of sulphates by organic matter and the subsequent liberation of sulphuretted hydrogen by free carbonic acid.

The comparatively small amount of sulphates in the water, the presence of organic matter in the rocks, the temperature of the water, which is quite low, would be in support of this view; as well as the fact that none of the surface rocks appear to contain sulphids—quite a large excavation having been made in the immediate vicinity without revealing any pyrites.

A few feet from this spring there rises another, apparently om the same formation, but containing no sulphur. The bot- m and sides of the spring are covered with a yellowish-white ating of separated sulphur; bubbles of gas rise at frequent tervals, and on reaching the surface burst, emitting the odor sulphuretted hydrogen.

The substances contained in the water are potash, soda, ammonia, lime, magnesia, iron, alumina, carbonic acid, chlorine, lphuric acid, sulphydric acid, silicic acid, organic matter, and ght traces of nitric and phosphoric acids.

The results of analysis were as follows:

Potash, - - - -	0·070	grains	per	gallon.
Soda, - - - - - -	7·588	"	"	"
Ammonia (NH_4O), - -	3·765	"	"	"
Lime, - - - - - -	2·125	"	"	"
Magnesia, - - - - -	0·946	"	"	"
Oxyd of iron and alumina, -	0·360	"	"	"
Carbonic acid, - - -	12·992	"	"	"
Silica, - - - - - -	0·983	"	"	"
Chlorine, - - - - -	1·293	"	"	"
Sulphuric acid, - - -	0·116	"	"	"
Organic matter, - - -	1·160	"	"	"
Sulphur, - - - - -	1·524	"	"	"
Total, - - -	32·910	"	"	"

These substances may be combined in the following manner:

Chlorid of sodium, - -	2·045	grains	per	gallon.
" potassium, - -	0·110	"	"	"
Carbonate of soda, - - -	11·119	"	"	"
" ammonia, - -	6·950	"	"	"
" lime, - - -	3·650	"	"	"
" magnesia, - -	1·987	"	"	"
Sulphate of lime, - - -	0·197	"	"	"
Oxyd of iron and alumina, -	0·360	"	"	"
Silica, - - - - - -	0·983	"	"	"
Organic matter, - - -	1·160	"	"	"
Sulphur, - - - - -	1·524	"	"	"
Carbonic acid, - - -	2·626	"	"	"
Total, - -	32·711	"	"	"
Add oxygen equiv. to chlorine,	0·199	"	"	"
	32·910			

The iron and alumina are given as they were present in the esidue dried at 356° F. In the water fresh from the spring they ppear to be in solution as organic salts. The amount of CO_2 ncombined in the above statement is almost precisely sufficient convert the carbonates of lime and magnesia into bicarbonates 2·566 grains per gallon being necessary for the conversion of the alkaline earths present into bicarbonates). The sulphur exists partly as sulphuretted hydrogen, and partly as alkaline sulphid. The results on carbonic acid and sulphur are too low since the gases were not examined at the spring. The total amount of sulphur was determined by oxydizing to sulphuric acid with aid of chlorine gas. From the total amount of sulphur, that amount corresponding to the sulphuric acid found by analysis was deducted and the remainder set down as sulphur. The carbonic acid was determined by adding weighed amounts of the water to chlorid of calcium that had been freed previously from carbonic acid.

The ammonia was determined by Boussingault's method. In general, the methods given in Fresenius's quantitative analysis were followed in the several determinations.

ART. XXIX.—*On the Nature of the Action of Light upon Iodid of Silver*; by M. CAREY LEA, Philadelphia.[1]

MUCH difference of opinion has long existed in respect to the explanation of certain phenomena of photographic action. In the vast majority of cases the action of light is a reducing one. Salts of iron, of uranium, and of other metals are reduced from a higher to a lower stage of oxydation, and the same is the case with the combinations of certain metallic acids, such as bichromates and tungstates. These phenomena present no difficulty. It is only when we come to the silver haloids that obscurity commences.

It is generally held, and there seems no reason to doubt it, that chlorid and bromid of silver undergo reduction when exposed to light. I shall therefore pass over these compounds, and discuss only the action of light upon the iodid.

In respect to this, two opposite opinions have divided those chemists who have seriously occupied themselves with the subject. Some believe the action of light on the iodid to be purely physical, others hold it to be connected with an absolute chemical change; some again holding this chemical change to be a reduction to a sub-iodid, others to metallic silver.

There can be no doubt that when iodid of silver is exposed to light in the presence of free nitrate of silver, it undergoes reduction. An examination of this reduced substance showed that it still contained iodine; when treated with nitric acid, a solution of nitrate of silver was obtained, with a product of yellow iodid of silver. If the iodid of silver, after exposure to light in the presence of free nitrate, is carefully washed, the free nitrate is thereby removed. Digestion with hyposulphite of soda removes all neutral iodid present, and the residue consists wholly of that portion of the iodid that has been altered by the light. If this altered substance were metallic silver, it would of course dissolve wholly in nitric acid. But as just said, this it does not do, but leaves behind yellow iodid of silver perfectly soluble in hyposulphite of soda. It is clear, therefore, that iodid of silver is reduced by the action of light, when free nitrate of silver is present, to sub-iodid. This sub-iodid is converted by nitric acid into nitrate of silver and iodid of silver. And I have found the same to be the case when *tannic acid* is substituted for free nitrate of silver, though the action is greatly slower: as in the former case a sub-iodid is formed.

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[1] In the following series of investigations it has been the object of the writer to endeavor to fix, with greater exactness, the obscure chemical and physical phenomena which form the basis of the photography of the day. The details have been published in the journals especially devoted to photography. It has been suggested to him to make a brief abstract of these studies in their essentially chemical and physical relations.

There is a question, however, far more difficult than these, to answer, and it is this: Does reduction of some sort invariably accompany the action of light upon iodid of silver? Is, or is not that action, in its essence a chemical action?

Before proceeding to investigate that question, another presents itself, demanding solution. It had been long held as an indisputable fact, which none had attempted to controvert, that perfectly pure iodid of silver was insensitive to light, and that sensitiveness only appeared when free nitrate of silver, tannic acid, or other "sensitizer" was present.

I soon satisfied myself that this asserted fact (for the explanation of which long discussions had taken place) had no existence whatever, and that pure iodid of silver was always sensitive to light. The long series of experiments, made with the most careful and multiplied precautions, and varied in many different ways, need scarcely be detailed here, except to say that I finally adopted, as most free from opening for cavil, the method of producing specular films of pure metallic silver on plates of glass: these were thoroughly iodized by very prolonged exposure to solutions of iodine, then thoroughly washed, and on these, invisible impressions made by light were developed without difficulty. The demonstration was so convincing that I have had the pleasure of seeing those who were the most earnest supporters of the old view, abandon it entirely.

I return then to the main question, which is: Does chemical decomposition necessarily accompany the production of an impression upon iodid of silver? In my opinion *it does not.* I hold that: 1. When perfectly pure iodid of silver, isolated, is exposed to light, it receives a physical impression only. 2. But that when certain other substances, for example, nitrate of silver, tannic acid, and perhaps many others, are present, then a chemical action, a reduction, does, or may, take place.

The second of these propositions is generally admitted; the first, on the contrary, is much contested. I shall therefore briefly state the results of the several series of investigations undertaken to arrive at a clearer view of the principles involved.

When pure iodid of silver, isolated, is exposed to light for a very brief period, an invisible, or latent image is produced, which by the action of a solution just ready to precipitate metallic silver, becomes evident.

If the action of light in producing an invisible image upon pure iodid of silver isolated be a chemical one, it is not possible that it should be destroyed except by chemical means.

A piece of glass supporting a film of pure iodid of silver isolated from all other substances was exposed for many hours to a strong sunlight. It was then placed in a dark closet for thirty-six hours, at the end of which time it was placed under a negative and exposed to light for two seconds. On pouring a developer over it, a clear bright picture instantly appeared. Thus the action of the sun for many hours had produced an impression which completely disappeared in thirty-six hours.

Now if the action of light is to reduce iodid to sub-iodid, how did this sub-iodid recover its lost proportion of iodine? The fact that the iodid was much more powerfully affected by a recent exposure of two seconds than by one which though thirty-six hours old, was many thousand times as long, and in light much more intense, seems fatal to the chemical theory. That theory holds that the production of a latent image is accompanied by a reduction. The plate in question then either should under the action of the developer have received a deposit all over, or else must have recovered its iodine. The latter case is not supposable, the other alternative did not take place, therefore the action of light could not have been chemical.[*]

In some cases, the action of light upon perfectly pure iodid of silver, isolated from all other bodies, may produce a visible image. This very remarkable fact, a description of which I published early in June last, and which has been since verified by other observers,[3] might seem to be inimical to the physical theory. A careful study proved the contrary. Pure iodid of silver, still moist, in considerable quantity, freshly precipitated and washed, was placed in a porcelain basin and exposed to sunlight. It instantly showed the slight darkening above referred to, but even after the action of the light and direct sunlight was continued for several hours, no reduced silver could be detected. The iodid after exposure was treated with dilute nitric acid, which restored the iodid to its bright yellow color. Now had this been effected by the action of the acid upon any sub-iodid formed by the action of light, then some portion of silver should have dissolved in the nitric acid. None however could be detected in it. It appears therefore that the effect of light upon pure iodid of silver, isolated from all bodies that can modify its action, is not a chemical one, whether its effect be visible or invisible, and that even when the action of light is prolonged to many thousand times the period sufficient for the production of a developable image, still no chemical alteration can be detected in the exposed iodid.

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[*] The only objection to this argument that I have seen made is one by Dr. Vogel, who remarks that in one case when actual chemical decomposition undoubtedly does take place, sensitiveness is recovered by repose in the dark. This is in the case of Obernetter's paper; paper imbued with perchlorid of iron and protochlorid of copper. Light reduces the iron salt, which again converts the copper salt to dichlorid; on applying a sulphocyanid a precipitate is produced on the parts altered by light. It is alleged that this capacity to precipitate disappears in a day. It is a sufficient answer that iron and copper are highly oxydable metals, and that the action of the light upon the above salts, though undoubtedly chemical in its nature, *is rapidly undone by the action of the air*, which, as is well known, quickly converts protochlorid of iron into oxychlorid, and dichlorid of copper into a green mixture of oxychlorid and cupric chlorid. This explanation—evidently the correct one in the case of the iron and copper salts—cannot be applied to a metal like silver which has no tendency to oxydize by atmospheric action. Even if this answer were not in itself sufficient, it is further to be remarked that there is no analogy in the two developments—the silver development being accompanied by no decomposition of the image, as is the case with the iron and copper mixture.

In addition to the foregoing, which are direct arguments, is to be placed the following indirect.

Believing that the action of light upon isolated pure iodid of silver is purely physical or molecular, it occurred to me to try whether another form of physical action, viz. *simple mechanical pressure*, could not be made to afford a basis for development. These experiments resulted in the formation of those curious effects which may be called *pressure images*. If a sensitive plate, ready for the camera, be pressed upon in any way, without removal from the dark room, and then be exposed to the action of a developing solution, all the parts so pressed upon will receive a heavier deposit of silver than the rest. This fact gives rise to various curious experiments.

If, for example, a piece of wood with lettering or figures cut out of it in open work be pressed forcibly on the sensitive film for a few seconds, and the plate then be developed the image of the lettering or open work will come out distinctly.

So if an embossed card be used, the embossed work will appear upon the film as soon as the developer has produced a deposit. The experiment may be varied in many ways: the material, so long as it is one which does not act chemically upon silver solutions, is unimportant; all that is required is a difference of pressure.[4]

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[3] See paper by Major Russell in Sutton's Notes, vol. xi, No. 245.

[4] These experiments have been since repeated by Girard with substitution of glass and rock crystal, with like results.

Chemists are very familiar with the fact that in cases of slow precipitation, the precipitate tends to follow the path which the glass rod used to stir the solution, has travelled over. The analogy which this and the development of a latent image present cannot be missed, and they are brought closer still by the pressure images just described. In all cases a molecular alteration takes place, and the molecules thus altered seem to have their attraction for exterior objects increased, and for each other and the surrounding homologous particles diminished.

This has been beautifully illustrated by some experiments lately published. A steel burnisher being drawn firmly over a plate of glass, yet so as not to scratch it or abrade the surface, the glass on examination by polarized light showed its structure altered by a line of color where the burnisher passed. This molecular effect slowly diminishes, and in a few days the particles return to their wonted state.

The foregoing experiment beautifully illustrates the changes of molecular condition which bodies are capable of undergoing. I have just remarked, that by drawing a blunt glass rod over the inside of a glass vessel, the parts so treated attract to themselves the particles of falling precipitates in a very striking manner. Often if a glass beaker containing a solution has been much stirred before a precipitate takes place, that precipitate will search out and develop, so to speak, all the invisible lines which were the path of the glass rod. This development of these invisible lines has a very striking analogy with the development of a latent photographic image, and the experiment just cited, reveals by polarized light a change of structure in those paths over which the body has been drawn. In the course of hours or days, this molecular change gradually passes away, and the body recovers its original condition, just as I have shown that when as in the case of pure iodid of silver, isolated, no reduction takes place, the effect of light gradually passes off, leaving the iodid free to receive a new impression.

It seems almost superfluous to say that what is here stated of the development of pressure images is not given by way of explanation, but as illustration and confirmation of the general view expressed.

Before concluding, a few words seem required as to what explanation is to be given to the ordinary process of Negative Photography.

In the foregoing I have reasoned on the action of light upon pure iodid of silver isolated. In the camera, light is made to act upon iodid of silver in a very different condition. It is in contact with nitrate of silver and with organic matter, and here bromid of silver is present.

I think it is evident from what has been already said, that in this case *several* images are formed, superposed as it were, on each other,—First, a physical image upon the iodid of silver. Secondly, if the exposure be sufficient, an image formed of sub-iodid of silver produced by the action of light upon the iodid and nitrate. Thirdly, there may be an image formed by the action of light upon the silver or its iodid in connection with the organic matter of the film (collodion or albumen). And, fourthly, if bromid or chlorid be present in the film, these may undergo reduction.

The separate nature of these images, or some of them, which may exist in the same film, interpenetrating each other, may be shown by the following experiment.

If an ordinary bromo-iodized collodion plate be exposed in the camera for the ordinary time and then be thrown into a dilute solution of acid pernitrate of mercury, the whole *of the iodid and bromid will be dissolved*, leaving a film just as clear and transparent as the glass on which it rests. Let now this be well washed, and then a developer of silver and reducing agent be applied in the ordinary way. An image will at once start out, which by care and redevelopment may be brought up to any strength desired. Now the acid nitrate of mercury has the property of quickly dissolving not only iodid and bromid of silver, but also sub-iodid and metallic silver. Consequently whatever basis was afforded by the first, second, and fourth sources as above enumerated, was completely removed. Nothing remained but the third, and it is not improbable that in some of the "dry processes" this third source is the principal basis of the picture, though evidently only a subsidiary one, in the ordinary "wet process."

Another form of this experiment consists in developing the picture *before* immersing it in the solution of acid nitrate, and then in leaving it but a short time in that solution, so that only the visible picture shall be removed, and the film of iodid and bromid be left.

Here all basis for development depending upon reduction is removed, and the production of the picture must depend wholly upon the first and third of the foregoing causes to the exclusion of the second and fourth.

Without having extended my observations to the *Daguerreotype*, I may remark that this process must be ranked along with those in which there is a reducing substance present, and therefore reduction may take place. The metallic back of the daguerreotype plate stands in the same relation to the iodid that tannin and other so-called sensitizers do. It seems probable therefore that in the case of the daguerreotype, there may be sub-iodid of silver produced, and this seems to correspond with Hunt's results, and those of other experimenters since his time.

In fact I have succeeded in showing the analogy between these processes by a new experiment which seems to establish it in a very striking manner. I impressed a wet collodion plate in the camera and then treated it with a pyrogallic solution in which proto-nitrate of mercury was substituted for nitrate of silver. The reducing tendency of the pyrogallic acid predisposes the mercury to precipitate, just as it does the silver, and this precipitate is attracted to the modified iodid, precisely as the precipitation of silver. The result is the development of a picture on iodid of silver by mercury *in the wet way:* this is precisely the intermediate stage between the ordinary collodion process in which a plate is developed by silver in the wet way, and the daguerreotype in which it is developed by mercury in the dry way.

This experiment, though affording no direct proof that the ordinary impression of light is a physical one, still tends to confirm that view. For if the chemical theory be true, it is a reduced image that attracts the precipitate of silver, and such an image would not be likely to attract a foreign metal. But if we conceive the phenomenon to be altogether a physical one, the dissimilarity of the precipitate from the surface on which it falls, imposes no more difficulty than the dissimilarity of the nature of the glass from that of the precipitate which is attached to those parts of the glass which have been pressed by the rod in stirring.

In fact it may be truly said that whenever one body is pressed against another, the particles of the body pressed against tend to have their attraction for each other diminished, and their attraction for external bodies, whether homogeneous or heterogeneous, increased.

I have endeavored in this brief review of a subject replete with difficulties, to show that the action of light upon pure iodid of silver isolated cannot be a chemical reduction:

1st. Because that effect, even when carried many hundred thousand times further than in the ordinary photographic processes, perfectly disappears in a few hours, spontaneously, under circumstances which render it impossible to suppose that iodine could have been restored to replace that which (had reduction taken place) must have been disengaged.

2d. Because, even where the action of light is prolonged many hundred thousand fold the ordinary time, no reduced silver nor sub-iodid can be detected as present.

3d. I have shown that another metal, mercury, is capable of developing these images as well as silver.

4th. I have endeavored to show that a purely physical cause, to wit, mechanical pressure, is capable of producing a developable impression, thereby answering the objection of the inadequacy of a physical influence to create a basis of development.

And finally, I may remark that although the chemical theory is supported by some distinguished chemists of the present day, I am not aware that there is a single well verified experiment which can be brought forward in support of that view. Had there been, I should have submitted it to a most careful study, my object being to arrive at the truth, not to support a theory. In the absence of such, I have been necessarily obliged here to confine myself to the affirmative side of the question, in support of the existence of a physical image, distinct from chemical reduction, and though often accompanied by it, yet never necessarily.

I cannot conclude these remarks without expressing my thanks for the kind assistance given me by my friend Mr. Thomas P. Shepard of Providence, Rhode Island.

Philadelphia, July 10, 1866.

ART. XXX.—*Observations on the origin of some of the Earth's Features;* by JAMES D. DANA.[1]

THE work by Mr. Vose on Orographic Geology, briefly noticed in our last number,[2] closes its general review of the different hypotheses with regard to the origin of the earth's features, with a brief statement of "conclusions." Among these conclusions are the following: that the deposition and subsidence of large accumulations of sediment produce the folding and compression of strata; that the alteration or metamorphism of sediments has arisen primarily from compression, "this force being resolvable into other actions that give rise to phenomena which seem to be due to heat and to chemical action;" and that "it is better philosophy" to attribute the results of metamorphism to this cause "than to a supposed central mass of fluid, gaseous emanations, and the like, that we know nothing about, which seem opposed by important facts, and which, from all we know, should act generally and not locally; and especially not in the regions of great accumulation which from their very thickness would seem to be most removed from any source of heat beneath the earth's crust." (p. 133.)

[1] For the writer's earlier discussions of this subject, see volumes ii, iii, iv, vii, xxii of this Journal, second series, the writer's Expl. Exp. Geological Report, and his Manual of Geology.

[2] Page 123, of this volume.

In thus holding that the "plication of strata has resulted from the subsiding of large masses of sediment" (p. 134), the author follows Professor James Hall, an abstract of whose views, with some criticisms, he gives on pages 47 to 55. The adoption of this hypothesis by an author treating professedly of geological dynamics prompts to an early consideration of its merits; and I propose to state in this place some of the objections that have occurred to me. These objections are in part alluded to by Mr. Vose, yet without allowing them their full weight.[1] The method also of metamorphism differs but little from that of Prof. Hall.

As Mr. Hall's views have not been explained in this Journal, I here cite a few paragraphs from the convenient abstract of them given by Mr. Vose, with others from the full exposition of the subject by Mr. Hall in the Introduction to the third volume of his Paleontology of New York.

"When large masses of sediment are spread along a sea-bottom, as originally along the line of the Appalachians, the first effect will be a yielding of the earth's crust beneath, and a gradual subsidence. We have evidence of this, first in the great amount of material accumulated; for we cannot suppose that the sea was originally as deep as the thickness of these beds: indeed, the ripple-marks, the marine plants, &c., prove that the sea in which these deposits were successively made was always shallow. The accumulation could thus only have been made by a gradual subsidence of the ocean-bed. The greatest depression would be along the line of the greatest accumulation; and, in the direction of the thinning margin, the settling would be less. By this process, as the lower side became gradually curved, rents and fractures upon that side would occur, while the compressed upper surface would be wrinkled and folded. The sinking down of the mass produces a great synclinal axis; and within this, whether on a large or a small scale, will be numerous smaller synclinal and anticlinal axes. And the same is true of every synclinal axis: it will contain still smaller synclinals, or, if the folding is not sharp enough to actually make the smaller folds, the tendency will still be to produce the various phenomena of compression (cleavage, &c.). That the subsidence was periodical, we have the best evidence in the unconformability of certain beds; showing that the lower one was bent and disturbed before the deposition of the upper one. This hypothesis explains why the mountain elevations in disturbed regions bear in their altitude a much less proportion to the thickness of the formations, than the hills in undisturbed regions do to the thickness of their beds. The great weight of the thicher beds has caused them to subside so much, that a larger part of their depth is now below the level of the sea."—*Vose*, pp. 48, 49.

"The direction of the waves formed by the settling of any wide area will be parallel to the great synclinal axis; a fact already stated in different language by Professor Rogers. The present mountain ranges are the evidences of the ancient oceanic currents, and thus the causes which determined the elevations existed long before the production of the mountains themselves. At no point, says Mr. Hall, between the Appalachians and the Rocky Mountains, could a mountain chain have been produced, because the accumulated materials were insufficient. The Rocky Mountains owe their greater height to a series of later deposits than those which cap the Appalachians; the White Mountains are covered with a later formation than that which covers the Green Mountains; the Alps are newer than the Jura; and, generally, if it is the original deposition of the materials that has produced mountains, then the greater the accumulation the higher will be the chain when it is finished."—*Vose*, p. 51.

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[1] See his note to page 48, and pages 53, 54.

"It nowhere appears that this folding or plication has contributed to the altitude of the mountains: on the other hand, as I think can be shown, the more extreme the plication, the more it will conduce to the general degradation of the mass, whenever subjected to denuding agencies." "The elevation [of the Appalachians] has been of continental, and not of local origin." "I believe, moreover, that this mountain chain, in its component parts, and in its mode of accumulation, and the process by which it has assumed its present position does not differ materially from other mountain ranges." If the fundamental rocks of the Alps are of Paleozoic age, and the sequence has been continued, even with some interruptions, to the end of the Jurassic period or later, it is no wonder that there are high summits, for the accumulation must have been enormous; and if to the Liassic and Jurassic we add the Cretaceous and Tertiary, we may get mountains of the elevation of the Himalayas. For I hold that no mountains of this elevation can occur without the long-continued accumulation of sediments; sediments not simply marking this altitude, but vastly more, for there is doubtless as much of the mass below the level of the sea as above it. This view we find applicable to the Appalachians, and it must be a necessary condition of mountain elevation."—*Hall's Pal. N.Y., Vol. III, Introduction.*

"We must look to some other agency than heat for the production of the phenomena [of metamorphism]; and it seems that the prime cause must have existed within the material itself, and that the entire change is due to motion, or fermentation, and pressure, aided by a moderate increase of temperature" produced by the sinking of the thickened mass to a level where the surrounding temperature was higher.—*Ibid.*, p. 77.

The hypothesis then is that the thickening of the deposits along the Appalachian region to 40,000 feet, more or less, which went on through the Paleozoic ages (and which was due mainly, as Mr. Hall holds, to material distributed by the northeastern oceanic current, now the cold Labrador current) ultimated in a subsidence of 40,000 feet, and in flexures, plications, fractures, metamorphism, trap dikes, and mountains; and that the same general principle was exemplified in the origin of the Rocky mountains and Andes, the Alps and Himalayas, and all other mountain elevations.

A. The fact of a subsidence in the Appalachian region accompanying the accumulation of the deposits is established by shallow-water markings in most of the successive strata, from the bottom to the top of the Paleozoic, as stated by Professor

Hall. And it must be admitted, further, that in other regions of the globe subsidence has, in most cases, attended similar accumulations. We do not question this postulate of Mr. Hall's. The Carboniferous formation of Nova Scotia is a case of the kind where the evidence is clear; for although 16,000 feet thick, it bears throughout proofs of its origin near the ocean's level, in dirt beds, coal seams, estuary deposits, and the like.[4] The 16,000 feet of thickness prove, therefore, 16,000 feet, approximately, of gradually progressing subsidence.

The subsidence connected with the formation of the successive thick deposits of sediments in the Appalachian region was, then, *a foot-per-foot movement;* that is, taking into view the grand result through the Paleozoic, there was a foot of sinking for a foot of accumulation. The sinking may have gone on paroxysmally, or with intervals of quiet, and of reversed oscillation, and unquestionably did so; and still it was, as a whole, for the long series of periods and ages, a foot-per-foot movement,—the whole amount of subsidence being gradually accomplished, and equalling at least the mean thickness of the deposits.

Now such easy sinking from mere gravity would require—

(1) A very yielding crust; and, therefore, a very thin one; with

(2) A perfectly mobile liquid beneath it. In fact, this foot-per-foot movement would demand that

(3) There should be no impediment, either in the crust, or in the density of the fluid below, to subsidence from added weight, and, at the most, only temporary checks to the movement.

Considering, then, the nature of rock, the laws of resistance, the form of the earth, the dependence of sinking in a floating body on its density as compared with that of the liquid; and further, that the sediment brought in *displaces its volume of water*, so that the weight it adds is only the difference of the two; what should be the thickness of the earth's rocky crust, and what its density, in order that it should be thus sensitive to the touch of sediments? Could there be the foot-per-foot movement under any degree of thinness? Surely the 800 miles of the mathematicians, or the densely viscid or the solid interior of some theorists, would give it no chance. And would a thickness of twenty miles, or of ten, or of five, allow of this no-resistance movement? The idea is obviously opposed to the very nature of the earth and its forces.

But suppose this subsidence through gravity to take place. It would occasion (1) some lateral pressure in the subsiding crust, which would react upon the crust around, tending to produce displacements where the crust was thinnest and weakest; and (2) pressure upon the interior liquid mass of the globe, which would be felt variously over the whole inner surface of the shell.

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[4] For details on this point, see Dr. Dawson's paper in the Jour. Geol. Soc. of London, No. 86 (May, 1866), p. 95.

The effects would, hence, be widely distributed, and be but feebly appreciable in any region, as will be seen on plotting to the scale of nature. A small part only of the action would tend to cause displacements over the region of the *thickened* (and thereby strengthened) sinking crust. Flexures on the scale of magnitude and number presented in the Appalachian region, grouped, as they are, most thickly over its middle and to the eastward, and fading out westward where the crust *has not one-fourth as great a thickness of sediments*, are opposed to the fundamental principle appealed to in the hypothesis.

We observe, further, that the larger part, at least, of the bold flexing of the Appalachian rocks took place toward, or at, the close of the Carboniferous age, *after* the Paleozoic deposits of Pennsylvania had been laid down; while, on the contrary, it should have very largely attended its progress, if sinking were the cause, since seven-eighths of this sinking had taken place before the era of the Coal-measures. Moreover, the metamorphism and the making of the mountains were mainly involved in the grand final result, and were a part of it.

We may well question whether the earth, as long as its crust was so sensitive to the weight of a layer of gravel, would anywhere be able to hold up mountains; for mountains have gravity as well as gravel beds or other sediments. We should hardly have expected, after a sinking had been going on in the Appalachian region for ages through simple gravity, a foot for a foot of deposits, that there, on that same yielding crust, the Appalachian mountains should have found a firm standing place, and especially before the vast Rocky mountain region, half the continent in breadth, was through with its sinking process; or that in Triassic and Jurassic times, the Green mountains should have kept their place on the west, and the high table lands on the east, when the crust was so weak below that the sands washed into the Connecticut valley by the hillside streamlets caused it to bend downward an inch for every inch of accumulation, till some thousands of feet of sandstone and of subsidence had been produced;[6] or that the more ancient ridges of the Alps should have been able to stand with uplifted heads while great Tertiary basins in Switzerland, and over the regions of the present Apennines and Pyrenees, and in other parts of Europe, were so thinly bottomed that a sinking, through the weight of gravel of no greater specific gravity than the rocks of the Alps, was going on by the thousands of feet; and that these same sinking basins should have next become the site of mountain peaks.

B. It has been remarked above that the hypothesis of Mr.

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[6] These Triassico-Jurassic beds of the Connecticut valley have a width of 20 miles or so, and extend from Long Island Sound at New Haven 120 miles to northern Massachusetts. Prof. Hall includes the region among the examples of subsidence from thickening accumulations of sediment. (See Paleontol. N. Y., vol. iii.)

Hall necessarily presupposes an exceedingly thin crust over the liquid interior. And yet the advocates of this hypothesis have for the most part rejected the idea that the cause of metamorphism lies, to any great degree, in the heat of this liquid interior. It is obvious, that with so thin a crust as the gravity-hypothesis demands (supposing any degree of thinness to meet its requirements) it would be superfluous to look to any other source for metamorphism, or for the material of trap dikes. It cannot be said of the central mass of fire by one advocating such an hypothesis, that it is a source of heat of which "we know nothing." A very thin shell over a sea of fire of extreme mobility is one of the essential conditions demanded by the hypothesis. Heat cannot come from *compression* occasioned by the slow action of gravity; for this produces none: and how can metamorphism?

C. Mr. Hall's hypothesis has its cause for subsidence, but none for the lifting of the thickened sunken crust into mountains. It is a theory for the origin of mountains, with the origin of mountains left out. Mr. Vose has some just criticisms on this point.

I do not now propose to present arguments in favor of the hypothesis which appeals to contraction from cooling as a cause of change of level. Although I have been inclined to give it the preference to others, I freely acknowledge that it has its difficulties. I would remark only on one point which may need an additional word of explanation.

All admit that the mountains of the globe are situated mostly along the border regions of the continents (taking these regions as 300 to 1000 miles or more in width), and that over these same areas the sedimentary deposits have, as a general thing, their greatest thickness. At first thought, it would seem almost incredible that the upliftings of mountains, whatever their mode of origin, should have taken place just where the earth's crust, through these sedimentary accumulations, was the thickest, and where, therefore, there was the greatest weight to be lifted. This difficulty Mr. Vose seems to regard as bearing especially against the hypothesis of contraction, while in reality it is the least of all obnoxious to it. Were the force causing elevation, under this agency, one acting directly *from beneath*, the gravity of the mass would prove an obstacle. But it is not so when the disturbing or uplifting force is *lateral* action or pressure from tension in the contracting crust. Under this agency the disturbances, and the mountain-making, find no great impediment in the thickness of the accumulations. Moreover, it is to be noted that the thick accumulations are produced just where oscillations and disturbances, or great yieldings in the crust, had been in progress through the long preceding ages (attending the accumulation of the sediments), and, therefore, just where such disturbances or yieldings were most likely to continue to occur through after time. Earthquakes show that even now, in this last of the geological ages, the same border regions of the continents, although daily thickening from the sediments borne to the ocean by rivers, are the areas of the greatest and most frequent movements of the earth's crust.

ART. XXXI.—*Contribution to the Chemistry of the Mineral Springs of Onondaga, New York;* by CHARLES A. GOESSMANN, Ph.D., Chemist to the Salt Company of Onondaga.

SOME of the characteristics of the Brines of Onondaga, N. Y., have already been illustrated by a series of analyses in two previous reports elsewhere published.[1]

The following pages may be considered as a continuation of the discussion on subjects of similar import, since I propose to treat here of certain peculiar features of the locality where these brines are found.

The majority of the wells, from which the supply of brine for the Onondaga Salt Works has been hitherto obtained, are located in a half circle around the southern and eastern shores of Onondaga Lake, north and west of the city of Syracuse. This entire district consists mainly of low lands, which are yet partly in a marshy state. They have been reclaimed in the course of time from the original lake bed, by natural and artificial drainage, and extend from one to one and half miles south of the lake. They are everywhere bounded by more or less abruptly rising grounds. These embankments, toward the east and west, at the southern end of the lake basin terminate, in several places, in hills of a hundred feet and upward in height, consisting chiefly of sand and gravel. In other quite frequent instances may be seen the outcroppings of the red and blue shale of the Onondaga Salt Group at a considerable elevation above the level of the lake. The Onondaga red and blue shale, in a crumpled condition, forms a clay of corresponding color; its present outcrops seem to have been originally covered with the same diluvial detritus which is spread so extensively over the northwestern part of the State. The shales of the Onondaga Salt Group underlie, also, the lowlands at the southern termination of the lake, dipping apparently at a rapid rate toward the center of the valley or its proximity, and are supposed (Beck) to form a natural underground basin in which the brine accumulates. This presumed basin is no doubt, filled in its lower portions with the same *debris* which covers the surrounding hills. Layers of sand, coarse gravel, loamy soil and hard pan are found here quite frequently, alternating with each other. Upon the deposit, within the low lands (mainly along the banks of the Onondaga Creek), rests a considerable amount of alluvial material of a loamy nature, sometimes rich in pebbles. Trunks of trees and hickory nuts, in a tolerable state of preservation, have been found at a depth of from twenty-five to thirty feet. These formations, particularly near the outcrops of the Onondaga shales, or at the termination of their surface drainage, are frequently found to be filled with an abundance of water of a peculiar saline character.

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[1] Report on the Brines of Onondaga, by C. A. Goessmann. Syracuse, N. Y., December 6, 1862.
Report on the Manufactory of Solar Salt, &c., by the same. Syracuse N. Y., December, 1863.

These waters sometimes contain only mere traces of chlorids, while in those from other similar localities in their vicinity a considerable amount of chlorid of sodium may be observed.

Among the questions to which these facts give rise, the following appeared to me of great interest:

First: Is there any relation between the chemical composition of the spring waters peculiar to the locality, and the brines?

Second: What chemical changes may result from their union, should their composition materially differ.

Third: Do the waters of the springs and the brines derive their characteristic qualities from soil or rocks of one and the same kind, though in different conditions; or do they both owe their peculiar chemical composition to entirely different sources; and if so, where are these sources located?

We may, for instance, suppose—adopting to some extent a view which has been advocated with much propriety in similar connections—that in the course of time an increased amount of percolation, or a certain order in the stratification, or a difference in the nature of the various layers, or particular advantages in drainage in general, may have favored a more rapid extraction of the more soluble compounds as chlorid of sodium, etc., in one locality than another. In fact, the well known and interesting discovery by A. Eaton,[1] of pseudomorphs of chlorid of sodium found in layers of a hardened clayish deposit—somewhat remote from the present brine-supplying district—contends strongly in favor of a previous gradual extraction of larger quantities of more soluble saline compounds (chlorid of sodium in particular) from the rocks and soil of the surrounding country.

The existing deficiency[2] of properly detailed geological records of numerous wells sunk at former periods is a deplorable fact, which seriously interferes with the discussions of the questions above proposed. For venturing at all, under such disadvantageous circumstances, on the third question, I trust I may be justified by the course I shall adopt, viz: presenting merely a few facts which have come within my observation. Future local geological investigations must decide upon their value.

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[1] W. Haidinger. Mittheilungen, etc., November 12, 1846. Wien. This Journal, xv, January, 1829.

[2] We are indebted to the Hon. George Geddes for a very valuable map of the outlines of the geology of Onondaga County.

In entering upon considerations of the above questions I selected two springs from an elevation several miles distant from the brine-bearing district, two in its immediate vicinity and a sample of average brine of the Syracuse district.

The samples were as follows:

a, From a well at the north of a hill where, formerly, numbers of pseudomorphs of chlorid of sodium had been found; the well terminated in a hard clayish shale.

b, From a well situated midway between the former and the brine-furnishing locality; the well terminated in the red clay of the Onondaga Salt Group. Both wells were at least from forty to fifty feet above the level of the lake.

c, From a spring within the brine-producing district, at a height of about ten feet above the level of the lake.

d, From a spring in close proximity to *c*, and at the same elevation.

e, A brine from the vicinity of springs *c* and *d*.

a. Water from a well (47 feet deep) on Willow street near the corner of Catharine street, in the city of Syracuse.

The well from which this water was collected is situated in the vicinity of one of those spots (James street height) where, some years ago, while workmen were engaged in grading James street, a considerable number of pseudomorphs of chlorid of sodium were found. Serpentine was also discoverd not far off. Two prominent hills, of which the most conspicuous is known as "Prospect Hill," intervene between that locality and the quite abrupt descent of the high grounds around the east and southwest side of the former lake bed—our present brine-furnishing district. In sinking this well, (Nov., 1863,) a layer of gravel-bearing loamy soil of ten feet thick was passed, then twelve feet of crumbled green shale, and lastly twenty-five feet of a hard light green clayish shale. The water usually stands fourteen feet. The ground perforated by this well is probably fifty feet above the level of the lake, and about two and a half miles from the nearest salt well.

One thousand parts of this water contained—

Calcium, - - - -	0·2302 parts.
Magnesium, - - -	0·0359
Sodium, - - - -	0·0101
Silica, - - - -	0·0050
Chlorine, - - - -	0·0156
Sulphuric acid, - - -	0·3442
Free carbonic acid, - - -	(not determined)

1000 parts of this water left at 200° to 212° F., 0·8906 parts of solid residue; one gallon would consequently leave 3·36525 grams or 51·9849 grains.

b. Water taken from a well sunk from the top of Prospect Hill, to a depth of seventy-five feet, i. e., the level of Salina street at the corner of Lock street. Prospect Hill lies about midway between the well which furnished the water for Analysis *a*, and the northwestern termination of the high embankment (30 or 40 feet) around the eastern and southeastern shores of Onondaga Lake and its adjoining low lands. Prospect Hill consists mainly of gravel and is covered with numerous boulders, and underlaid with the red clay of the Onondaga Salt Group. The gravel is here and there interspersed with layers of cemented gravel (hard pan) and of a red loamy soil. These layers are of varying extent and apparently without any order of succession. The water subjected to analysis was taken from the first quantities drawn from the well soon after its completion (April, 1863). Quantitative tests for carbonic acid and iron, being under existing circumstances of no value, were omitted.

One thousand parts of this water contained—

Calcium, - - - -	0.52838
Magnesium, - - -	0·03954
Sodium, - - - -	0·00821
Sulphuric acid - - -	1·02660
Chlorine, - - - -	0·01268
Silica, - - - - -	0·00450
Iron, - - - - -	(not determined)
Free carbonic acid, - - -	" "

1000 parts of this water left, at 200° to 212° F., 2·0080 parts of solid residue. The residue from one gallon would therefore be 118·8602 grains.

c. This water was taken from a spring near the Syracuse Pump House, on the bank of Onondaga Creek, upon grounds which had at one time been occupied by vats for the manufacture of Solar Salt. The spring issues from an embankment near the place where the Onondaga Creek has forced its way through the southern enclosure of the low lands around the south shore of Onondaga Lake. This vicinity is rich in springs of similar character.

This peculiar water was found at a depth of from forty to fifty feet, where loamy soil or hard-pan had intervened between it and the brine proper. Brine had, no doubt, more or less access to some of these springs, if only by surface percolation. The spring which furnished me with the sample for this analysis was somewhat protected by a tight barrel in which it was purposely enclosed; and the barrel was several times emptied before collecting the sample for examination, (Sept., 1863).

ne thousand parts of this water contained—

Calcium, - - - -	0·35265
Magnesium, - - -	0·07620
Sodium, - - - -	4·50454
Sulphuric acid, - - -	0·64379
Chlorine, - - - -	6·95266
Silica, - - - -	0·00490
Free carbonic acid, - - -	(not determined)
Carbonate of protoxyd of iron (traces),	" "
Bromine (traces), - - -	" "

000 grams left, at 200° to 212° F., 13·0340 grams of solid idue; one gallon would consequently leave 49·2405 grams, or 7309 grains.

l. This sample of water was taken from a spring about twenty- to thirty feet distant from the last one, *c.* The spring is en- sed in a tight wooden tank of 10 to 12 feet deep, and issues the foot of an embankment from thirty to forty feet high. elevation above the level of the lake corresponds nearly with t of the spring *c.*

000 parts of this water contained in solution—

Calcium, - - - -	0·28147
Magnesium, - - -	0·07700
Sodium, - - - -	4·01378
Silica and alumina, - -	0·01770
Sulphuric acid, - - -	0·48150
Chlorine, - - - -	5·30918
Bromine, - - - -	0·00232
Free carbonic acid, - - -	(not determined)
Carbonate of protoxyd of iron, -	" "

1000 parts of this water left, at from 200° to 212° F., 11·7382 rts of solid residue. One gallon would therefore leave 5·07402 grains of residue. This residue contained in com- ation, 0·1150 parts of carbonic acid.

The brine for this analysis was taken from a salt well in vicinity of *c* and *d*, (July 30, 1863).

Calcium, - - - -	2·25005
Magnesium, - - -	0·36799
Sodium, - - - -	61·06500
Potassium, - - - -	0·05720
Iron, - - - -	0·02123
Chlorine, - - - -	96·36355
Bromine,[4] - - - -	0·02080
Sulphuric acid, - - -	3·39550
Free carbonic acid, - - -	(not determined)

000 grams of this brine contained 164·243 grams of saline

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[4] Iodine, traces.

residue; one U. S. gallon[5] would therefore contain 9586·9891 grains.

The brines of Onondaga, though differing somewhat in concentration, vary but slightly in regard to the relative proportions of their component parts; and the analytical statement just made, will be found to meet all practical requirements.

In 1000 parts of	Willow st. well.	Prospect Hill well.	Mineral water.	Mineral water.	Syracuse brine.
are contained	*a.*	*b.*	*c.*	*d.*	*e.*
Calcium,	0·2302	0·5284	0·3526	0 2815	2·2500
Magnesium,	0·0359	0 0395	0·0762	0 0770	0·3679
Sodium,	0·0101	0·0082	4·5045	4·0137	61·0650
Potassium,*	....	....	....	....	0·0572
Sulphuric acid,	0·3442	1·0266	0·6437	0·4815	3·3955
Chlorine,	0·0156	0·0127	6·9526	6·3092	96·3635
Bromine,*	....	....	....	0·00232†	0·0208
Silica,*	0·0050	0·0045	0·0049	0·0177†	....
Carbonic acid,*	....	....	....	0·1150‡	....

* In all cases where no figures are given no quantitative tests have been made.
† Silica and alumina. ‡ Combined in the residue left at 200° to 212° F.

Although, unquestionably, much significance must be conceded to the fact that the same group of elements form the most prominent features of the analytical results, there nevertheless existed some doubts whether a mere succession of extractions of the same strata or the same kind of rocks, etc., would suffice to explain satisfactorily the peculiar nature of the various liquids; a view which appears still more conspicuous when these component parts are arranged in the combinations most likely to be present in each of the waters.

Having arrived at this point, I preferred to institute some inquiry in regard to the action of certain compounds toward each other under circumstances similar to those I should be obliged to take into consideration if a discussion of the second question should promise encouraging results; and in this connection I would call particular attention to the earlier statements of C. B. J. Karsten,[6] and the results given in the highly interesting publications of T. Sterry Hunt,[7]—the former treating mainly of the nature and changes of the brines—the latter more especially of the chemistry of natural waters in general.

My experiments were in some cases designed to give merely an idea of the degree of certain changes under given circumstances. It appeared to me of importance to ascertain—

[5] One United States gallon is equal to 241 cubic inches or 3778·625 grams or 58318 grains.

[6] C. B. J. Karsten, Salinenkunde, Berlin, 1843.

[7] T. Sterry Hunt, Chemistry of Natural Waters, this Journal, March, July and September, 1865.

I. *How does carbonate of magnesia act upon sulphate of lime in the presence of free carbonic acid?*—To test the degree of this action, I adopted the following course. I mixed in a suitable vessel ten grams of well washed carbonate of magnesia, twenty-five grams of sulphate of lime, (gypsum,) and five hundred cubic centimeters of distilled water; which mixture I afterwards treated for a short time each day during four weeks, with well washed carbonic acid gas, so as to secure a constant supply of bicarbonate of magnesia to the solution of gypsum. The solution thence resulting, after being separated by filtration from the white residue, was equal to four hundred and fifty cubic centimeters. I placed it in a flat glass dish, and left it to gradual evaporation at the ordinary temperature.

After a few days rest the bottom of the vessel began to be covered with a small amount of sediment. No further noteworthy change took place, until the whole liquid gradually formed into a solid crystalline mass, consisting in the main of a net-work of needles. The solution, being tested before its solidification, was of a slightly alkaline reaction; the crystals resulting from its evaporation were transparent, yet crumbled readily to a white powder when exposed to dry air.

One hundred parts of an apparently air-dried sample of the saline mass, contained—

Carbonic acid, - - -	1·6721
Sulphuric acid, - - -	31·4803
Calcium oxyd (lime), - - -	0·7988
Magnesium oxyd (magnesia), -	17·0327

One hundred parts of the same mass, heated to a dull red heat, lost 51·0593 parts of its weight; leaving therefore 48·9407 parts of a white residue.

These analytical results, if considered in connection with the general properties of the original liquid as above stated, and the consequent solid crystalline residue, tend to prove that the solid matter may, with some propriety, be considered as consisting of

Carbonate of lime, - - -	1·4268
Basic hydrated carbonate of magnesia $3(MgO, CO^2)+(MgO, HO)$, -	2·4911
Sulphate of magnesia (MgO, SO^3+7HO),	96·8020

As the whole amount of the saline compound obtained in the experiment just described, was 22·605 grams, we learn that about 14·5 grams of gypsum had been decomposed, while some of the carbonate of lime (or more properly, bi-carbonate) left in the originally diluted liquid, had been separated during the progress of its concentration as carbonate of lime. The apparently slow decomposition of gypsum must be attributed to the limited solubility of that compound. It is not unreasonable to presume that carbonic acid, under pressure and at common temperature, would alter the degree of action above illustrated.

II. *How does carbonate of magnesia act upon sulphate of lime in the presence of free carbonic acid and chlorid of sodium?*—In this investigation I proceeded thus: I weighed into a glass bottle 31 parts of commercial carbonate of magnesia, 86 parts of gypsum, 58 parts of chlorid of sodium, and 3000 parts of distilled water, and treated the whole mass with carbonic acid gas for several weeks, as described in a former experiment. The filtrate, separated from the undissolved white mass, which mainly consisted of gypsum and carbonate of lime, besides mere traces of carbonate of magnesia, was evaporated to dryness at 180° F., and the residue subsequently extracted with absolute alcohol. The alcoholic extract contained (beside a very small quantity of chlorid of sodium), 0·4370 grams of chlorid of magnesium, while the residue left after the extraction by alcohol contained 0·0570 grams of oxyd of calcium, 0·2243 grams oxyd of magnesium, and 0·6860 grams of sulphuric acid. This proved the presence of 0·6532 grams sulphate of soda, 0·4773 grams sulphate of magnesia, beside 0·1018 grams carbonate of lime, and 0·1236 grams carbonate of magnesia, with part of the chlorid of sodium unchanged. These results indicate,

1. That gypsum, carbonate of magnesia, and carbonic acid in the presence of chlorid of sodium, form chlorid of magnesium, sulphate of soda, and carbonate of lime.
2. That at a certain higher temperature, the sulphate of soda and chlorid of magnesium partly re-transform into sulphate of magnesia and chlorid of sodium.
3. That the solubility of the gypsum governs the degree of decomposition.

(To be continued.)

Art. XXXII.—*A new Meteoric Iron, "the Colorado meteorite," from Russel Gulch, Gilpin Co., near Central City, Colorado Territory;* by J. Lawrence Smith, Prof. Chem. in University of Louisville.

I have known of the existence of a new meteoric iron from Russel Gulch in Colorado, for about two years, but it was only recently that it passed into my hands. I first heard of it in the possession of Mr. Fisher of New York, who subsequently turned it over to Prof. C. F. Chandler of Columbia College, New York, who kindly submitted it to me, as I am furnished with the necessary means for cutting up and scrutinizing thoroughly this class of bodies.

The mass of iron is accompanied with the following label: "Meteoric iron found in Russel Gulch, Feb. 18, 1863, by Mr. Otho Curtice. Weight 29 lbs. Brought to New York, Feb. 1864."

The mass measures in its extreme length, breadth, and thickness, 8½×7¼×5½ inches. It is perfect in all parts except at one extremity, and, as stated above, weighs 29 lbs.

This iron is one of medium hardness, with the density 7·72, and when cut through was found to contain a few small nodules of iron pyrites. It is attacked readily by nitric acid, and gives bold Widmannstättian figures without very sharp angles. It resists the action of the air and moisture very well, and is consequently but little altered on the surface. No siliceous minerals could be traced in any of the crevices. On analysis its composition was found to be—

Iron, - - - -	90·61
Nickel, - - - -	7·84
Cobalt, - - - -	·78
Copper, - - -	minute quantity.
Phosphorus, - - -	·02
	99·26

I have not made any further observations in relation to the presence of copper in meteoric iron since 1852, when I called attention to it. Since then I have become more confirmed in the opinion, then first expressed, that copper would be found in all meteoric irons; this has been the result of examinations of many well known meteoric irons, and all new ones that have come under my examination.

One or two grains of the iron is all that is necessary for the examination, if it be done carefully; but four to five grains had better be used. Dissolve the iron in chlorhydric acid, and if necessary add a little nitric acid; it is as well at all times to add a drop or two at the end of the operation. Evaporate away the excess of acid, add water, precipitate with sulphuretted hydrogen until there be an excess of gas in the solution; throw on a filter and wash with water containing a little HS, dry the filter, burn in a porcelain crucible, treat the residue with a little *nitromuriatic acid*, and evaporate to dryness, with the addition of a drop or two of sulphuric acid; treat the residue with water, when the introduction of a clean plate of iron will cause a deposition of the copper with all its characteristic properties.

ART. XXXIII.—*On Gay-Lussite from Nevada Territory;* by B. SILLIMAN.

IN September, 1864, I visited the body of saline water known as Little Salt Lake, situated near Ragtown, about a mile and a half south of the main emigrant road to Humboldt. It fills the bottom of a deep funnel-shaped depression in the Desert plain. The form and other peculiarities of this depression suggest a volcanic origin. It is distinctly crater-shaped, with the outline a double ellipse, made apparently by the coalesence of two craters; the larger is to the north, and has a diameter of about a mile and a half. The whole length north and south is somewhat greater than that from east to west in the larger division. The water-surface is about 200 feet below the lip of the crater, which is elevated somewhat above the general level of the plain. The slope of the converging sides is steep, varying from 25° to 45°; the approach to the water is therefore difficult, except at one or two points where an oblique footpath has been worn. There is a narrow margin or beach, varying from a few yards to a hundred feet or so, covered with shoal water, and the shore then plunges off to very deep water. There is a small island in the northern or larger division of this lake, also surrounded by shoal water. The section of the slope shows a series of beds of volcanic materials, lapilli and ashes, mixed with boulders or masses of black basalt, and concretions from thermal springs. The shores on the west side are also skirted with calcareous matter, and there is a steady flow of water from numerous small springs of fresh water into the lake. One of these springs is a copious fountain of excellent drinking water. The water of the lake is very saline. Its taste is salt, bitter, and decidedly alkaline. Its effect on the skin in bathing is that of a solution of an alkaline carbonate, and its odor is strongly marine. The rocks are encrusted with saline matters resulting from the evaporation of the waters.

The surface of the lake swarmed with small ducks; and divers sandpipers, cranes and other aquatic birds were on its shores. Myriads of larvæ of a species of fly (equally abundant at Mono lake) swarm in the shallow waters of the shore, but no fish appear to live in it. The water is so dense that a swimmer floats on it like a cork. There are no thermal springs now active in the lake, the temperature of which is normal.

Gay-Lussite.—A large stick floating on the shore and covered completely with an incrustation of yellowish-white crystals tubular in form attracted my attention. A careful search along the shores failed to detect others of a like kind adhering to the rocks, and it occurred to me that probably the waters being diluted near the shore were not dense enough to deposit these crystals; and that if we could reach the island we might find them there, where, from the absence of fresh water springs, the saline solution of the lake would be more dense. This conjecture was fully verified. Mr. Semple, my secretary, succeeded in reaching the island in a very insecure boat, where he found the shores completely incrusted with beautiful clusters of these crystals, whose acute edges cut the naked feet. We secured an abundant supply of this rare and interesting mineral which, I believe, had not before been recognized in the United States. No other crystallized mineral was discovered.

The Gay-Lussite obviously has its origin from the reaction of the salts of soda and lime with which the waters are abundantly charged, and being very slightly soluble is readily deposited in these situations where the density of the water is maintained or increased by solar evaporation.[1] Hence it does not occur along the shores where the marginal springs of fresh water dilute the solution. The flow of these springs does not in summer fully replace the solar evaporation, as is evident from the water-line retiring slightly from its winter level.

This interesting lake has no outlet. It has plainly been a point of volcanic activity in modern geologic times, its eruptions being confined to mud, ashes, pumice and lapilli. It is one of a considerable number of similar phenomena with which the Great Basin is dotted, and of which Mono lake, on the western margin of the Desert, is the most remarkable. The bottom of the ancient lake whose waters have left beautiful terrace-lines along the sides of the Humboldt and other mountain ranges, when these were either marginal shores or islands of this great mediterranean sea of fresh or brackish waters, is every where strewn with dead fresh-water modern shells, chiefly univalves.

ART. XXXIV.—*On crystals of Gay-Lussite, from Nevada Territory;* by JOHN M. BLAKE.

THE crystals of Gay-Lussite here described were obtained by Prof. B. Silliman in 1864, at Little Salt Lake, near Ragtown, Churchill Co., Nevada. The crystals differ strikingly from those measured and described by Phillips (Phil. Mag., April, 1827) in the proportional development of the planes as is shown by comparison with the figures given by Phillips, and by Descloizeaux (Ann. Ch. Phys., 3d series, vol. vii, p. 489).

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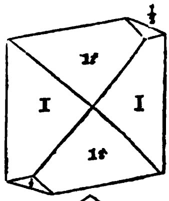
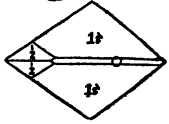
[1] Gay-Lussite has been made artificially by J. Fritzsche, by mixing eight parts by measure of a saturated solution of carbonate of soda with one of a solution of chlorid of calcium of 1·130—1·150 specific gravity.—*J. f. pr. Ch.*, xciii, 339.

The planes observed by Phillips were *I*, *O*, *iì*, 1*í*, 1*ì*, and ½; and the monoclinic axes, calculated from his measurements, are $a:b:c=1{\cdot}444:1{\cdot}489:1$; $C=78°\,27'$. Of the above-mentioned planes, *iì* was not detected on the Nevada crystals. But there is a plane, not before mentioned, *ií*, which can be seen when the crystal is placed in the proper position to reflect the sunlight, and it then appears to be made up of numerous microscopic planes. The same was true of 1*í*. These two planes, giving no definite reflected image of the sun, were approximately measured by noting the points at which the light was reflected with the maximum intensity.

In my trials I found the cleavage parallel to planes *I* perfect; parallel to *O* less perfect, giving a reflected image with a strong light. Specimens in the Yale Cabinet from near Lake Maracaibo, South America, showed the same composite character of the planes; but the effloresced condition of the specimens prevented any exact comparison with them.





The following are the angles obtained: the faces are mostly too feebly polished to afford results nearer than a degree. The angles are given in the order in which they were obtained in the several zones.

Zone 1st: *I* on *I*, 69° 25′; *I*, 180° 20′; *I*, 247° 50′.

Zone 2d: 1*ì* on 1*ì*, 69° 30′; *O*, 123° 20′; 1*ì*, 177° 50′, 179° 40′; 1*ì*, 249° 30′; *O*, 304°; 1*ì*, 0°.

Zone 3d: *I* on 1*ì*, 43° 20′; ½, 71°, small; *I*, 180° 20′; 1*ì*, 221° 20′; ½, 249° 40′, small.

Zone 4th: *I* on 1*ì*, 53° 10′; *I*, 179° 20′, 180° 10′; 1*ì*, 231° 20′; *I*, 359°, 0°.

Zone 5th: *I* on ½, 52° 50′; *O*, 96° 10′; *I*, 179° 10′; ½, 231° 15; *O*, 274° 20′.

Zone 6th: *O* on 1*ī*, 50°; *ií*, 101°; *O*, 178° 40′; 1*í*, 228° 30′? *ií*, 281°?

The following are Phillips's measurements, arranged in three zones, containing all of his observed planes:

Zone 1st: *I* on *iì*, 34° 25′; *I*, 68° 50′; *I*, 180°.

Zone 2d: 1*ì* on *iì*, 35° 15′; 1*ì*, 70° 30′; *O*, 125° 10′; 1*ì*, 180°.

Zone 3d: *I* on 1*ī*, 42° 15′; ½, 69° 55′; 1*ī*, 110° 20′; *I*, 180°.

New Haven, Ct., Jan. 1866.

ART. XXXV.—*On the Structure and Habits of Anthophysa Mülleri* Bory, *one of the sedentary monadiform Protozoa;* by H. JAMES-CLARK, A.B., B.S.

DURING the last five years, and more especially within the latter eighteen months I have been engaged largely upon an investigation of the relations of the monadiform animalcules to the zoöspores of the true *Algæ;* and of all the numerous instances of the former that I have more or less thoroughly studied I have never met with one which could be said to bear but a very moderate resemblance to the latter. I refer to the true *Algæ.* I scarcely need add that I mean by this to except those doubtful forms which seem to be related to *Volvox* and *Gonium*, such as *Pandorina*, *Protococcus*, *Stephanosphæra*, *Chlamidococcus*, &c.

Those who have become accustomed to these creatures, and have learned to look upon them, through long years of patient study, as old and familiar friends, know well the value of using the best lenses that the opticians of the present day can afford; and never doubt for a moment the utter worthlessness of an opinion which is founded upon a few fitful glances through a so-called ordinary working microscope. There is no other group of animals which so essentially seems to need the prolonged devotion of a specialist as the *Protozoa;* and above all the lower members of that grand division. To write a monograph upon any single one of these flagellate forms may seem like devoting a volume to the structure and phases of a dot in a sunbeam, but no good microscopist need be told that the optical instruments of the present day are no less efficient than was the scalpel in the hand of Cuvier when he displayed to the world the organization of the larger and more elevated animals which he found on the southern shores of France.

Moreover it is particularly desirable that elaborate investigations should be made, and unstinted minutiæ set forth in illustrations and descriptions, because there are yet among zoölogists those who suppose that there is so little in the organization of Protozoa that no tangible characters can be found by which they may be typified, or assimilated in a group by themselves.

The taxonomic relations of the organs of the *Infusoria Flagellata* have received so little attention from investigators that there is no small difficulty, with our present knowledge of them, in tracing the typical plan which is so eminently exemplified among the *Ciliata.* I hope I shall be pardoned, therefore, if I attempt to give a strict topographical view of the positions of the various organs of one among the most lowly of the whole group of animalcules.

A considerable portion of the second volume of the great work of Messrs. Claparède and Lachman, "Etudes sur les Infusoires," &c., is occupied by a discussion of the animality of certain doubtful forms of Monad-like infusoria. The tests which these authors offer as determinatives of the zoölogical relations of the forms in question are the possession by them of a contractile vesicle and the introception of food. By means of either the one or the other of these criteria they succeed in satisfying themselves that the *Volvocina*, *Astasiæ* (*Euglenæ* included), and the *Dinobrya* are true animals; but in regard to other forms they are unable to decide. Among those which are left in the latter category, there is a singular infusorian which, as is usually supposed, was originally named *Epistylis vegetans* by Ehrenberg, and *Anthophysa Mülleri* by Bory de St. Vincent. Dujardin gives a scarcely recognizable figure of it in the atlas of his work on Infusoria; but very properly places it among the monadiform animalcules. This is done, however, upon its general resemblance to the latter—alike undetermined at that date as to their animal nature—and not because he had by direct observation decided it to be a genuine animal. The figures of Cohn (Mikroskopische Algen und Pilze. Nov. Acta Acad. Cæs, Leop, 1854. Taf. xv, fig. 1–8) are not much better than those of Dujardin.

Habitat and general appearance.—I have been so fortunate as to determine the animality of *Anthophysa* by both of the tests above mentioned; and there rests not the least doubt in my mind that this infusorian is as truly a member of the zoölogical kingdom as any of the well known Protozoa. I would state, for the information of those who are not acquainted with the habits of this animalcule, that it is quite common among the fresh water weeds. It may be most advantageously studied when it is attached to *Myriophyllum* or *Ceratophyllum*; a small piece of the tip of the filiform leaf, of either, which seems to be covered by an irregular, floccose deposit, usually affording abundant specimens.

Under a low magnifying power this floccose matter appears to consist of clusters of very jagged, irregularly branching and contorted, semitransparent, intertwined stems and projecting, tapering and flexible twigs. Each of the tips of the latter sustains a single, more or less globose mass of spindle-shaped bodies, which radiate from a common center of attachment; and are kept in a constant agitation by the spasmodic jerks of a long, stout, usually rigid, arcuate filament, with which the free end of each one is endowed. The whole bristling mass revolves alternately from right to left and from left to right; whirling upon its slender pivot with such a degree of freedom that one might almost suspect that it merely rested upon it, and had no truer adhesion to it than the juggler's top to the end of the *baton* upon which it spins. The largest of these twirling groups contains as many as fifty fusiform bodies, but most frequently not more than half that number are grouped together; and from this they vary in decreasing quantities down to only one or two upon each filamentous twig. In the last instances the bodies are comparatively quiet, scarcely moving out of focus at each spasmodic twitch of the arcuate filament. On this account, and because they offer an unobstructed view, the latter are by far the most available as objects for the investigation of their internal organization.

The relationship of the individual monads to the whole colony must, however, be studied where they are more numerously congregated; since, as will be shown presently, each monad sustains a definite relation to every other one, and to the twig to which it is attached. The larger colonies are frequently to be found swimming freely, with a rolling motion similar to that with which *Volvox* progresses. As a natural concomitant to this fact, twigs are to be met with, here and there, which do not bear anything at their tips. The colonies seem to break away very easily; and on this account the specimens should not be lifted out of the water when transferring them to the watch-glass or whatever sort of observing-trough is used.

Form, &c.—The adult monads have a truncate fusiform shape, and are slightly, but quite appreciably flattened on two opposite sides; so that in an end view they appear to be broadly oval transversely. The attached end tapers gradually to a point; and on this account it is difficult to determine where the body ends and the twig begins. All of the members of a group radiate from a common point of attachment, to which they adhere by their tapering filamentous ends. The free end is truncate, but one corner of it,—as if in continuation of the line along which the opposite flattened sides meet,—projects in the form of a rather blunt triangular beak. At the inner edge of the base of this beak lies the mouth, to which the former—as frequent observation has proved—acts as a lip or prehensile organ when food is taken into the body. The prevailing tint is a more or less uniform light gamboge, without the least trace of an eye-spot of any color.

A most singular uniformity prevails in the arrangement of the several members of a group. Each monad is attached to its mooring in such a position that its flattened sides lie parallelwise with those of its nearest neighbor; and the beak projects from that corner of the head which is most distant from the twig. To give a full idea of the peculiarity of this arrangement it must be stated here that the rigid, arcuate, spasmodically twitching filament mentioned above is attached close to the mouth, and invariably curves away from the beak, and consequently always toward the pedicel of the colony. One is forcibly reminded by this of the systematic relation of some of the flowers of *Labiatæ*, with their stamens projecting far beyond the upper lip of the corolla. The globose heads of the *Menthæ* are particularly good examples for illustrating this similitude.

Prehensile organs.—The only motile organs which this animalcule possesses are preëminently prehensile in character; and their apparent appropriation for the office of propulsion, when a colony breaks loose from its attachment, I can scarcely doubt is an accidental one, inasmuch as the arcuate cilium continues its spasmodic twitching without any apparent deviation from its usual mode of action.

There are two cilia, of very unequal size, attached to the truncate end of the body. The larger one of these has already been mentioned casually, as a *rigid, arcuate filament.* It does not taper, but has a uniform thickness from base to tip, and is about half again as long as the body. It arises near the base of the triangular beak, but appears to be separated from the latter by the intervening mouth. When quiet it appears like a bristle, and projects in a line with the longer axis of the body; at the base bending slightly toward the beak, and then sweeping off in a moderate but distinct curve in the opposite direction, so that on the whole it presents a long drawn out sigmoid flexure. The plane of this curve lies in strict parallelism with the plane of the greater diameter of the body; in fact it may be said to be a direct continuation of it. It does not appear to have the character of a *flagellum*, except when assisting the smaller cilium to convey the food to the mouth; and then it lays aside its rigid deportment and assumes all the flexibility and wavy vibration of the prehensile organ of an *Astasia.*

The smaller cilium is an excessively faint body, and almost defies the detective powers of the highest objectives. This is partly due to its almost incessant activity; for when it is quiet, or nearly so—which happens when food is passing into the mouth—it becomes comparatively quite conspicuous under a one-eighth of an inch objective. It is scarcely as long as the greater diameter of the truncate end of the body. It arises close to the base of the larger cilium, but whether on the right or left, or nearer or more distant from the mouth than the latter cannot be said positively. Most frequently it was observed to be flexed in the same direction as its companion; and occasionally it seemed to be quite evident that it was attached nearer to the mouth than the latter. It is highly flexible and vibrates with great rapidity in what appears to be a gyratory manner.

The mouth.—This organ is never visible except when food is passing through it. It then may be seen that it lies close to the beak, which acts as a sort of lip by curving over the introcepted particles as they pass into the body. The mouth is highly distensible; at times allowing particles as wide as two-thirds the greater diameter of the body to pass in without any apparent extra effort. It seems undeniable that it possesses discriminative powers in regard to the quality of its food. This one may readily judge of for himself, by seeing the unerring precision with which the particles of floating matter are thrown, by the spasmodic incurvature of the larger flagellum, against the mouth, where, if they are not swallowed, they are detained but for an instant by the smaller cilium, quickly adjudged to be worthless, and then thrown off with a twirl of the organ which held them in temporary abeyance. If, however, the captured morsel proves to be agreeable, the larger cilium assists the operations of the smaller one, and the lip, by abruptly bending itself at its point of attachment and laying its basal part across the food, and pressing it into the mouth, while the terminal portion is kept in a constant wavy vibration, and curved toward the posterior end of the body. This is usually done in three or four seconds and then the cilia return to their usual positions, while the introcepted edible passes toward the center of the body, and is there immediately enclosed in a digestive vacuole. For a while the food dances about in this vacuole with a very lively motion, but finally it subsides into quietude.

The contractile vesicle.—There is a two-fold difficulty in discovering the presence of this organ. In the first place it is comparatively quite small, and secondly it pulsates so slowly that it is very rarely possible to see it contract twice in succession between any two of the abrupt, lateral deviations of the body, which the spasmodic twitchings of the arcuate flagellum produces. On this account it has not been possible to determine the precise rate of its systole and diastole. It seems to contract from three to four times a minute. It lies near the surface, about half way between the two ends of the body, and nearly midway betwixt the two extremes of its greater diameter. At the completion of its *diastole* it has a circular outline, and appears like a clear colorless vesicle in the midst of the yellowish tissue of the body. Upon contraction it disappears and leaves no trace of its presence. The *systole* progresses slowly, as in *Anisonema* (*A. sulcata* Duj.? and *A. nov. sp.*), *Cyclidium* (*C. nov. sp.*), and *Phacus pleuronectes* Duj.; and in this respect contrasts strongly with the same process in *Heteromita fusiformis* Jas.-Clk., *Astasia tricophora* Clap., and *Cryptomonas* (*C. nov. sp.*), in which the last half of the systole is very abrupt and marked.

The stem.—In addition to what has already been said of the general appearance of this part of the organism it may be added that the older and basal portions of the branches are flat, and have a distinct longitudinal, irregular striation; to all appearances made up of the older, laterally agglutinated twigs. The youngest, terminal portions of the branches which, under the name of twigs, have been described in this paper as the immediate supporters of the colonies of monads, are evidently tubular. They appear to be as flexible as a spider's thread, and are usually quite irregular in outline, and in the calibre of the canal which permeates them. The wall of these tubular twigs is quite thick, and is alike rough on the exterior and interior faces. The substance within the tubes appears homogeneous, but whether it is solid or fluid could not be determined. The oldest part of the stems is of a reddish brown color, but as they taper off into branchlets they gradually assume a gamboge color, and finally terminate in scarcely colored twigs.

Reproduction by fissigemmation is the only method of propagating individuals which I have observed. As a preliminary to this process the monad gradually loses its fusiform shape, and assumes at first an oval contour and finally becomes globular. During this transition both of the prehensile cilia become much more conspicuous than usual, and the body develops a closely fitting hyaline envelope about it; thus passing into a sort of encysted state. The contractile vesicle, however, does not seem to cease its pulsations during this period, and moreover it becomes quite conspicuous. This arises mostly from the fact that the body is in a nearly quiet state, and allows the observer to obtain a prolonged and undisturbed view of it. Unfortunately the rate of the pulsations of this organ was not ascertained when the following observations were made, because the whole time was occupied in watching and drawing the various and rapidly changing phases of self-division.

After the body assumes a globular shape, as above mentioned, both the larger and smaller cilium seems to be undergoing a change, and becomes indistinct in outline.[1] Presently two larger flagella burst upon the view, apparently by the longitudinal splitting of the previously single one of the same kind, and rapidly separate from each other by the broadening of the body, and leave between them the smaller cilium. The latter at this time appears much thicker than usual, and seems to be composed of two closely approximated, parallel threads. By this time the contractile vesicle has also divided into two, which lie closely side by side.

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[1] In a new fresh-water genus (see note 2) of sedentary, monadiform Protozoa—possessing two contractile vesicles, and only the *sigmoid flagellum*, the latter arising within a deep bell-like flange, or projecting rim which embraces the anterior end of the body—this arcuate filament disappears altogether by a sort of withering down from tip to base—reminding one of the shrivelling of the end of a cotton thread in the flame of a lamp—preliminary to the commencement of the longitudinal fissigemmation of the body and its bell-like flange; and then the new flagellum of each resultant of self-division grows out in about twenty minutes.

[2] *Codosiga: κωδων* a bell, *σιγαω* to be silent. *C. pulcherrima*, n. sp. Body obliquely obovate, and tapering at its posterior end into a slender pedicel; truncate and abrubtly constricted in front where the base of the bell meets the body. Sigmoid, arcuate flagellum as long as the body and bell. The two contractile vesicles in the posterior third of the body; superficial, large, and quite conspicuous; each contracting, alternately with the other, once in about half a minute. Bodies attached, in groups of from two to eight, by their pedicels to the tip of a slender stem; erect or divergent, but not pendent. Mouth at the base of the flagellum, i. e., terminal. Anus near the mouth. No eye-spot. Bell slightly flaring; half again deeper than broad; fully as deep as the length of the body; highly contractile. Color of the body—excepting the hyaline bell—pedicels and stem deep yellow. Common on fresh-water weeds about Cambridge.

At this moment the time noted in one series of observations was 2.30 P.M. By 2.35 P.M. the larger flagella had separated still farther, and the smaller cilium had split into two very conspicuous filaments; as yet, however, attached to a common point of the body. From this time forth to the completion of the process of fissigemmation all of the cilia kept up a slow vibration, in which they undulated from base to tip with a sort of snake-like motion. By 2.45 P.M. the body had become quite appreciably broader than long; the contractile vesicles were widely separated, and the smaller cilia had left between them a considerable space, and each one had approximated quite near to the base of a larger flagellum. At 2.50 P.M. the body had become nearly twice as broad as long, and the space between the two pairs of cilia was nearly twice as great as in the last phase, and considerably depressed in the middle, so that the body had a broadly cordate outline. By 2.52 P.M. the posterior end of the body—at a point a little to one side of the spot where it was attached to the pedicel—was also slightly indented, so that in outline it presented a guitar-shaped figure, each rounded half of which bore a pair of unequal cilia, and contained a contractile vesicle. In one minute more the contraction had increased to such an extent that the body was divided about half way through. By 2.54 P.M. the animal had a dumb-bell shape, and the pedicel was attached to one of the segments near the point of constriction. Still the process went on very rapidly, and by 2.55 P.M. the new bodies were widely separated, but still attached to each other by a mere thread. At 3 P.M. the body which was attached to the pedicel was left alone, and its companion swam away to seek a new attachment, and build up its stem.

To the last moment the hyaline envelope remained about the segments, and in fact so long afterwards that time and circumstances did not allow me to ascertain its final disposition. I would remark, however, that when the ovate bodies of the half grown monads are contracted temporarily into a globular shape, they appear identical—excepting that they lack the hyaline envelope—with these recently fissated forms. In all probability, therefore, the latter lose their envelope and assume the shape of the former.

As to the development of the stem I think it quite certain that it grows out from the posterior end of the body. The best proof of this is that I have frequently found a monad—especially in the condition of the one which I described above as breaking loose from its companion—nearly sessile upon a clean spot, and attached by a very short, faint, film-like thread. From this size upward I had no difficulty in finding abundant examples as gradually increasing in diameter as they did in length; thus furnishing a pretty strong evidence that the stem grows under the influence of its own innate powers, and is not therefore a deposit emanating from the body of the monad, except, perhaps, as far as it may be nourished by a fluid circulating within its hollow core.

Cambridge, Mass., May 21, 1866.

ART. XXXVI.—*Address of Prof. DeCandolle to the recent Botanical Congress in London.*[1]

IN order to derive the full advantage from a meeting of so many lovers of science, horticulturists and botanists, brought together from all parts of Europe, it is necessary that the common object for which they have met should be perfectly understood.

It devolves on me, who am called upon to preside (an honor of which I feel myself unworthy), to point out the bond which unites us, and of which perhaps you have at present but a vague, and, so to speak, an intuitive perception.

In my opinion, we are not here merely as amateurs to satisfy our curiosity. The proof of which is, we are here assembled to listen to discussions, instead of wandering about the fairy-like garden of the Exhibition. Evidently we seek something more than a mere flower show, and that something is, in my opinion, instruction. It is not sufficient for horticulturists merely to see —they must also study and reflect; neither is it sufficient for botanists to observe details minutely; they must also see the plants on a large scale and in grouped masses. The connection of practice with theory, and of art with science, is acknowledged to be indispensable; and in accordance with this prevalent opinion we here affirm, by our presence in this room, the necessary union of botany and horticulture. The aim of my brief observations will be to call to mind how they aid each other, and to show how much more they might do so. If I am not mistaken, it will follow from the facts to which I shall allude, that our united efforts, scientific or practical, modest though they appear, contribute to increase the well-being of man, in all conditions and in all countries.

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[1] The first meeting of the Botanical Congress was held in the Raphael Room of the South Kensington Museum on Wednesday, May 23, at 11 A. M., Prof. DeCandolle in the chair. A very large meeting, including almost all the British and foreign botanists and horticulturists present in London were assembled to hear the President's address.

1. *The advantages of horticulture to botany.*—Let us first mention the services that horticulture renders, or may render, to botany. Without being myself a horticulturist, I affirm or recognize them willingly, the advancement of science rendering it necessary to have recourse to all its collateral branches.

We no longer live in those times of illusion, when botanists merely occupied themselves with European plants, or with a few from the East, and, from a spirit of caution rather than from ignorance, pictured to themselves all distant countries as possessing much of the same general vegetation, with a few uncommon or exceptional species. A century of discovery has made known the extreme variety in the floras, the restricted limits of many species, and the complicated entanglement of their geographical distribution. To see all the different forms of vegetation of the world, would be to realize in a degree the history of the Wandering Jew; besides, with this constant travelling, where would be the opportunities for that reflection or study which create true science?

The traveller is too much exhausted in warm countries, too distracted in those temperate regions favorable to active life, and his faculties are too much benumbed in the colder regions, to enable him to devote himself to minute researches with the lens or the microscope, or even to sketch or properly describe that which he has gathered. He sees, in passing, a crowd of things, but he can scarcely ever stop to enter into details, especially of those that present themselves in rapid succession. Rarely can he see the fruit and flower of a species at the same time, and it is quite impossible for him to study their complete development during the whole year. The notes taken by the most intelligent naturalist are so affected by these fatal circumstances, that it is seldom they add anything to that which a dried specimen can teach the sedentary botanist.

It is horticulture, then, which brings before us a multitude of exotic plants in a condition best adapted for study. Thanks to the variety of species it accumulates and successfully cultivates, the botanist can investigate the most difficult questions, and pursue his researches in families whose genera are not indigenous in Europe. In the herbarium, more minute observations can be made than is generally supposed; nevertheless, for certain researches, it is absolutely necessary to have the living plant, particularly for those relating to the relative disposition, the origin and development of the several organs, as well as for studying the curious phenomena of fertilization, the movements and direction of the stem, leaves, and parts of the flowers. Horticulture has done much to advance the progress of physiological botany, but it still has much to do. The most remarkable experiments of physiologists—viz., those of Hales, Duhamel, Knight—have been made in gardens. Also the long series of experiments of the younger Gaertner, and, more recently, of M. Naudin, on hybridization, which relate to the cardinal subject of the species. As much may be said of the numerous trials which are made, in horticultural establishments, to obtain new races or varieties. These have a great scientific importance, and it is undoubtedly the horticulturists who are the teachers of botanists on these subjects.

It appears to me, however, gardens can be made still more useful in carrying out physiological researches. For instance, there is much yet to be learned on the mode of action of heat, light, and electricity upon vegetation. I pointed out many of these deficiencies in 1855, in my "Géographie Botanique Raisonnée."[1] Ten years later Mr. Julius Sachs, in his recently published and valuable work on physiological botany,[2] remarks much the same deficiencies, notwithstanding that some progress has been made in these matters. The evil consists in this, that when it is desired to observe the action of temperature, either fixed or varied, mean or extreme, or the effect of light, it is exceedingly difficult, and sometimes impossible, when observations are made in the usual manner, to eliminate the effects of the constant variations of heat and light. In the laboratory it is possible to operate under more exactly defined conditions, but they are rarely sufficiently persistent; and the observer is led into error by growing plants in too contracted a space, either in tubes or bell-glasses. This last objection is apparent when it is wished to ascertain the influence of the gases diffused in the atmosphere around plants, or that of the plants themselves upon the atmosphere.

Place plants under a receiver, and they are no longer in a natural condition; leave them in the open air, and the winds and currents, produced at each moment of the day by the temperature, disperse the gaseous bodies in the atmosphere. Every one is aware of the numerous discussions concerning the more or less pernicious influence of the gases given off from certain manufactories. The ruin now of a manufacturer, now of a horticulturist, may result from the declaration of an expert; hence it is incumbent on scientific men not to pronounce on these delicate questions without substantial proof.

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[1] Pages 46, 49, 57, and 1346.

[2] Handbuch der Experimental-physiologie de Pflanzen, 1 vol. in 8vo. Leipzig, 1865.

With a view to these researches, of which I merely point out the general nature, but which are immensely varied in details, I lately put this question[4]—"Could not experimental greenhouses be built, in which the temperature might be regulated for a prolonged time, and be either fixed, constant, or variable, according to the wish of the observer?" My question passed unnoticed in a voluminous work where, in truth, it was but an accessory. I renew it now in the presence of an assembly admirably qualified to solve it. I should like, were it possible, to have a greenhouse placed in some large horticultural establishment or botanic garden, under the direction of some ingenious and accurate physiologist, and adapted to experiments on vegetable physiology; and this is, within a little, my idea of such a construction:—

The building should be sheltered from all external variations of temperature; to effect which I imagine it should be in a great measure below the level of the ground. I would have it built of thick brickwork, in the form of a vault. The upper convexity, which would rise above the ground, should have two openings—one exposed to the south, the other to the north—in order to receive the direct rays of the sun, or diffused light. These apertures should each be closed by two very transparent glass windows, hermetically fixed. Besides which, there should be, on the outside, means of excluding the light, in order to obtain complete darkness, and to diminish the influence of the variations of temperature when light was not required. By sinking it in the ground, by the thickness of its walls, and by the covering of its exterior surfaces with straw, mats, &c., the same fixed degree of temperature could be obtained as in a cellar. The vaulted building should have an underground communication with a chamber containing the heating and the electrical apparatus. The entrance into the experimental hothouse should be through a passage closed by a series of successive doors. The temperature should be regulated by metallic conductors, heated or cooled at a distance. Engineers have already devised means by which the temperature of a room, acting on a valve, regulates the entry or exit of a certain amount of air, so that the heat regulates itself.[5] Use could be made of such an apparatus when necessary.

Obviously, with a hothouse thus constructed, the growth of plants could be followed from their germination to the ripening of their seeds, under the influence of a temperature and an amount of light perfectly definite in intensity. It could then be ascertained how heat acts during the successive phases from sowing to germination, from germination to flowering, and from this on to the ripening of the seed. For different species various curves could be constructed to express the action of heat on each function, and of which there are already some in illustration of the most simple phenomena, such as germination,[6] the growth of stems, and the course of the sap in the interior of certain cells.[7] We should be able to fix a great number of those minima and maxima of temperature which limit physiological phenomena. Indeed, a question more complicated might be investigated, toward the solution of which science has already made some advances, namely, that of the action of variable temperatures; and it might be seen if, as appears to be the case, these temperatures are sometimes beneficial, at other times injurious, according to the species, the function investigated, and the range of temperature. The action of light on vegetation has given rise to the most ingenious experiments. Unfortunately, these experiments have sometimes ended in contradictory and uncertain results. The best ascertained facts are, the importance of sunlight for green coloring, the decomposition of carbonic acid gas by the foliage, and certain phenomena relating to the direction or position of stems and leaves. There remains much yet to learn upon the effect of diffused light, the combination of time and light, and the relative importance of light and heat. Does a prolonged light of several days or weeks, such as occurs in the polar regions, produce in exhalation of oxygen, and in the fixing of green matter, as much effect as the light distributed from 12 to 12 hours, as at the equator? No one knows. In this case, as for temperature, curves should be constructed, showing the increasing or diminishing action of light on the performance of each function; and as the electric light resembles that of the sun, we could in our experimental hothouse submit vegetation to a continued light.[8]

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[4] Géographie Botanique, 1855, pp. 49 and 1346.

[5] See the electrical apparatus of M. Carbonnier, exhibited at Chiswick in 1837, figured in the "Flore des Serres et Jardins," vol. xii, Miscell. p. 184.

A building such as I propose would allow of light being passed through colored glasses or colored solutions, and so prove the effect of the different visible or invisible rays which enter into the composition of sunlight. For the sake of exactness nothing is superior to the decomposition of the luminous rays by a prism, and the fixing the rays by means of a heliostat. Nevertheless, a judicious selection of coloring matters, and a logical method of performing our experiments, will lead to good results. I will give as proof, that the recent most careful experiments concerning the action of various rays upon the production of oxygen by leaves and upon the production of the green coloring matter, have only confirmed the discoveries made in 1836, without either prism or heliostat, by Professor Daubeny,[9] from which it appears that the most luminous rays have the most power, next to them the hottest rays, and lastly those called chemical.

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[6] Germination under different degrees of constant heat, by Alph. de Candolle, in the "Bibliothèque Universelle de Genève" (Archives des Sciences), Nov. 1865.

[7] If the curves had not been constructed, the data for their construction are, at least, dispersed throughout our books. I will cite, for instance, the growth of a scape of Dasylirion, as observed by M. Ed. Morren (Belgique Hortic., 1865, p. 322). The figures there given are not favorable to the accepted notion, that the growth of tissues is more active by night than by day.

[8] The apparatus which produces the most persistent and vivid light is the magneto-electric machine, based on the development of induction by magnetism, as discovered by the illustrious Faraday. The galvanic pile is replaced by a steam-engine of low power, which sets in motion a wheel furnished with magnets (Bibl. Univ. de Genève, Archives Scientif., 1861, vol. x, p. 160). The working of this machine is inexpensive, but, unfortunately, the magnets are very costly. This system has already been applied to two lighthouses—that at the South Foreland, and to that of the "Société l'Alliance," at Havre—in consequence of the experiments of MM. E. Becquerel and Tresca.

Dr. Gardner in 1843, Mr. Draper immediately after, and Dr. C. M. Guillemin in 1857,[10] corroborated by means of the prism and the heliostat the discovery of Dr. Daubeny, which negatived the opinions prevalent since the time of Senebier and Tessier, and which were the result of erroneous[11] experiments. It was difficult to believe that the most refrangible rays—violet for instance, which acts the most on metallic bodies—as in photometrical operations, should be precisely those which have least effect in decomposing the carbonic acid gas in plants, and have the least effect over the green matter in leaves. Notwithstanding the confirmation of all the experiments made by Dr. Daubeny, when repeated by numerous physicists and by more accurate methods, the old opinions, appearing more probable, still influenced many minds,[12] till Mr. Julius Sachs, in a series of very important experiments, again affirmed the truth.[13] It is really the yellow and orange rays that have the most power, and the blue and violet rays the least, in the phenomena of vegetable chemistry; contrary to that which occurs in mineral chemistry, at least in the case of chlorid of silver. The least refrangible rays, such as orange and yellow, have also the twofold and contrary property, such as pertains also to white light, and which produces the green coloring matter of leaves or bleaches them, according to its intensity. It is these, also, which change the coloring matter of flowers when it has been dissolved in water or alcohol.[14] Those rays called chemical, such as violet, and the invisible rays beyond violet, according to recent experiments, confirmatory of those of ancient authors—those of Sebastian Poggioli, in 1817,[15] and of C. M. Guillemin—have but one single well-ascertained effect, that of favoring the bending of the stem toward the quarter from which they come more decidedly than do other rays; yet that is an effect perhaps more negative than positive, if the flexure proceeds, as many still believe, from what is going on on the side least exposed to the light.[16]

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[9] Daubeny, Philos. Trans., 1836, part 1.

[10] Dr. Gardner, Edinb. Phil. Mag., 1844, extract in French in La Biblioth. Univ. de Genève, February, 1844; Draper, Edinb. Phil. Mag., September, 1844, extract ib., 1844, vol. liv; Guillemin (C. M.), Ann. Sci. Nat., 1857, ser. 4, vol. vii, p. 154.

[11] Senebier, Mém. Phys. et Chim., ii, p. 69; Tessier, Mém. Acad. Sci., 1783; Gilby, Ann. de Chimie, 1821, xvii; Succow, Commentatio de lucis effectibus chemicis, in 4to, Jena 1828, p. 61; Zantedeschi, cited by Dutrochet, Compt. Rend. Acad. Sci., 1844, sem. 1, p. 853.

[12] As a proof of the persistence of the old opinion, I will quote a phrase of Professor Tyndall's, in his most clear and interesting treatise "On Radiation," (London, 1865,) p. 6:—"In consequence of their chemical energy, these ultra-violet rays are of the utmost importance to the organic world." I do not know whether the author had in view an influence of the chemical rays over the animal kingdom; but, according to certain passages of Mr. Sachs, I doubt if they have more power over animals than they have over plants; besides, Professor Tyndall did not concern himself with these questions; he was content to explain admirably the physical nature of the various rays.

[13] The researches of Mr. Sachs first appeared in the Botanische Zeitung: they are collected and condensed in the remarkable volume called Handbuch der Physiologischen Botanik, vol. iv, Leipsig, 1865, pp. 1 to 46.

The effect upon vegetation of the non-visible calorific rays at the other extremity of the spectrum have been but little studied. According to the experiments we have on this subject, they would appear to have but little power over any of the functions; but it would be worth while to investigate further the calorific regions of the spectrum by employing Dr. Tyndall's process, that is, by means of iodine dissolved in bisulphid of carbon, which permits no trace of visible light to pass.

How interesting it would be to make all these laboratory experiments on a large scale! Instead of looking into small cases, or into a small apparatus held in the hand, and in which the plants cannot be well seen, the observer would himself be inside the apparatus, and could arrange the plants as desired. He might observe several species at the same time, plants of all habits, climbing plants, sensitive plants, those with colored foliage, as well as ordinary plants. The experiment might be prolonged as long as desirable, and, probably, unlooked-for results would occur as to the form or color of the organs, particularly of the leaves.

Permit me to recall on this subject an experiment made in 1853 by Professor von Martius.[17] It will interest horticulturists now that plants with colored foliage become more and more fashionable. Prof. von Martius placed some plants of *Amaranthus tricolor* for two months under glasses of various colors. Under the yellow glass the varied tints of the leaves were all preserved. The red glass rather impeded the development of the leaves, and produced at the base of the limb yellow instead of green; in the middle of the upper surface, yellow instead of reddish-brown, and below, a red spot instead of purplish-red. With the blue glasses, which allowed some green and yellow to pass, that which was red or yellow in the leaf had spread, so that there only remained a green border or edge. Under the nearly pure violet glasses the foliage became almost uniformly green. Thus, by means of colored glasses, provided they are not yellow, horticulturists may hope to obtain at least temporary effects as to the coloring of variegated foliage.

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[14] Sir John Herschell, Edinb. Phil. Journ., January, 1843.

[15] S. Poggioli, Opuscoli Scientifici, quoted by Dutrochet, Compt. Rend. Acad. Sci., 1844, sem. 1, p. 850.

[16] The rather confused and questionable explanations, founded on the notions of Dutrochet, of the existence of a deoxydizing power on the brightest side, clash with the fact that the blue, indigo, and violet rays, the least powerful for deoxydizing tissues, are the most powerful in causing them to bend.

[17] "Gelehrte Anzeige," München, Dec. 5, 1853.

The action of electricity on foliage is so doubtful, so difficult to experiment upon, that I dare hardly mention it; but it can easily be understood how a building constructed as proposed might facilitate experiments on this subject. Respecting the action of plants on the surrounding air, and the influence of a certain composition of the atmosphere upon vegetation, there would be by these means a large field open for experiments. Nothing would be easier than to create in the experimental hothouse an atmosphere charged with noxious gas, and to ascertain the exact degree of its action by day and by night. An atmosphere of carbonic acid gas might also be created, such as is supposed to have existed in the coal period. Then it might be seen to what extent our present vegetation would take an excess of carbon from the air, and if its general existence was inconvenienced by it. Then it might be ascertained what tribes of plants could bear this condition, and what other families could not have existed, supposing that the air had formerly had a very strong proportion of carbonic acid gas.

Until horticulture can supply physiology with such convenient means of experiment, it, in the meantime, advances descriptive botany by the valuable publications it issues. The greater part of the old works with plates, such as "Hortus Eystettensis," "Hortus Elthamensis," &c.; also those of Ventenat, Cels, Redouté, &c.; the Salictum and Pinetum of the Duke of Bedford; and more recently the "Rhododendrons of the Himalaya," by Dr. Hooker; the works of Bateman, Pescatore, Reichenbach fils, on Orchids; and many others I could name, would never have existed, had there not been rich amateurs either to edit or buy them.

It is horticulture that has given us the longest series of illustrated journals that have ever been published; and here I must do justice especially to the English horticulturists. No doubt the science of our time requires a larger amount of analytical details than is contained in the plates of the "Botanical Magazine," "Botanical Register," "Andrews' Repository," "Loddiges'

Botanical Cabinet," "Sweet's British Flower Garden," "Paxton's Magazine and Flower Garden," and other English journals; but what a number of forms are thus fixed by the engravings in these books, and what a fund of valuable documents for consultation they afford. One must admire the "Botanical Magazine," commenced in 1793, continued from month to month with an exemplary regularity, and which is now at its 5580th plate. Not only has it always represented rare and new species, but it has ever been conducted on a simple and uniform plan, which renders it convenient to consult.

The series of plates is unique from the very beginning. Each plate has its number, and each article of letter-press refers only to one plate, by which means the quotations from the work are rendered brief and clear. Many editors have not understood the advantage of this simple arrangement. They have varied their titles, their series, their pagings; they have affixed to their plates numbers, then letters, then nothing at all; the end of which is (and this ought to serve as a warning for the future) that the more they have altered and complicated the form of their journals, the shorter time they have lasted.

How is it that these purely bibliographical details cause in us such sad recollections? Of the men just mentioned, who have rendered such eminent service to botany and horticulture, England has lost three during the year 1865—Sir Joseph Paxton, Dr. Lindley, and Sir William Jackson Hooker.[18] I should certainly fail in what is expected of me if I did not express, in the name of the foreigners attending this meeting, our deep regret at such serious losses. We know them all by their writings, and many among us have known personally the distinguished men I have mentioned. Their names follow us at each step in this the scene of their labors. If we admire the boldness of construction of the iron domes that characterize modern buildings, we think of the Crystal Palace, of Chatsworth, and of the humble gardener who became a great architect. If we visit the beautiful establishment at Kew, we see everywhere around us proofs of the indefatigable activity of Sir William Hooker. Lastly, if we ask the origin of the garden of the Royal Horticultural Society at Kensington, we are told it is only a development of that at Chiswick, where Lindley stood preëminent by his knowledge and his energy; and of that Society where botanists of my age found in their youth such valuable encouragement in their studies.

The names of Sir William Hooker and of Dr. Lindley, thanks to their special works, will ever remain distinguished in science. These two botanists have, moreover, been directors of horticultural journals, and of great horticultural establishments, and since their influence has been so fully acknowledged by practical men, I shall have little trouble in showing that science is as useful to horticulturists as horticulture is to botanists—and this will form the second part of my discourse.

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[18] Since these lines were in the printer's hands, British science has sustained a severe loss in the death of the truly amiable and learned Professor W. H. Harvey, of Dublin, so well known by his works on *Algæ*, and on the botany of South Africa. I cannot refrain from expressing our sense of this great bereavement.

2. *The advantage of botany to horticulture.*—The principles of vegetable physiology are what horticulturists and agriculturists usually study in books on botany. They do not always find direct answers to their questions; but they can draw from them certain rules, certain ways of experimentalizing and reasoning, which saves them from falling into many errors. Should some ridiculous idea be promulgated by some ignoramus or charlatan, it is by an appeal to the general rules of physiology that a practical man may at once reject them, or, at least, hold them in distrust. On the contrary, innovations, if in harmony with the principles, may be, and I will even say ought to be, readily accepted.

Do not let us put too much faith in the lucky results of experiments made absolutely by chance. It is with some of these experiments as with dreams and presentiments—if they come true once in a thousand times they are talked about, otherwise they are passed over and forgotten. Besides, it must be said, men nearly always are guided by theories; but the theories of the ignorant are often absurd and without foundation, while those of educated men are based on probabilities, or on an accumulation of facts.

Conjointly with physiology, botanical geography shows the distribution of plants all over the globe, their struggle with the elements, their migrations, and already raises a portion of the veil which covers the obscurity of their origin. All this ought to offer a real interest to horticulturists. We are beginning to have the power of expressing in figures the effect of each climate upon vegetation; consequently, the possibility of a given species enduring the mean or extreme climatal conditions of that country to which it is desired to introduce it. Already we can show, in the clearest manner, the analogy between the vegetation and climate of certain regions, widely separated the one from the other, and point out in which cases new attempts at cultivation should be tried or where they should be discouraged. A celebrated geologist was able to say, beforehand, there is gold in such a part of New Holland; and gold was found there. We can also say, the olive tree and the cork oak will succeed in Australia; the eastern and temperate region of the United States is favorable to the growth of Chinese plants, more particularly to that of tea; and we can assert that that part of America included between San Francisco and the Oregon territory will, one day, supply wines as varied and as excellent as those European ones produced between Portugal and the Rhine.

It is a singular fact, that the two principal beverages of the civilized world, wine and tea, which produce similar stimulating effects, but which to a certain extent are the substitutes one for the other in different countries, present also in the mode of cultivating them the most marked resemblances and differences. The vine and the tea-plant succeed best on stony, barren hill-sides, of which they sometimes increase the value a hundred-fold. According to the exposure, the soil, the cultivation and manner of preparing the produce, wine and tea are obtained of unquestionable excellence; while the neighboring crops, but a short distance off, may be more or less ordinary in quality. The two shrubs require a temperate climate, but the vine needs heat and no rain during summer, while the tea-plant requires rain and but little summer heat; the result of which is, that these two species are almost geographically incompatible. Vine-growing countries will never produce tea, and *vice versâ.*

But you will say, these examples belong rather to agriculture, and concern neither botany nor gardens. I maintain the contrary. It is science, in the present day, which points out what plants to cultivate, and into what countries to introduce them. Horticulture makes the trial, with infinite pains. If successful, the young plants are submitted to the less refined treatment of agriculture. Before the happy introduction of Cinchonas into British and Dutch India could be effected, botanists were required to collect, distinguish, and carefully describe the various species of American Cinchonas; horticulturists were then called on to make cuttings, gather the seeds, raise the young plants, transport and establish them in another part of the world; and so at last they were passed over to the care of the agriculturists. The coffee-plant did not spread gradually from Arabia to India, from India to Java; nor was it the American colonists who brought it from its original country to their fazendas or haciendas. The shrub was first described by botanists, and was afterwards introduced by the Dutch into a garden at Batavia; from thence it was taken to the Botanical Garden at Amsterdam, from whence a specimen was sent to the King of France in 1714. DeClieu, a naval officer, transplanted it from the garden at Paris to the French colonies in America. A multitude of such instances might be named. In the present day science has progressed, practical men avail themselves of it, governments and nations have abandoned those mistaken ideas in accordance with which it was supposed that a cultivation advantageous to one country was injurious to others. Hence we may hope to see, before long, useful species planted in all regions where they can thrive, to the great advantage of mankind in general.

One of the most evident effects of science has been to create in the horticultural public a taste for varied and rare forms. Formerly in gardens there were only to be found certain kinds of plants which dated back to the time of the Crusades, or even of the Romans. The discovery of the New World did not produce a change in proportion to its importance; perhaps because horticulturists did not travel enough, or acquaint themselves with those countries whose species were most suitable for cultivation in Europe. Botanists, fortunately, were more ambitious. Their collectors were numerous and daring. They enriched their herbaria with an infinitude of new forms, and published works upon exotic plants, such as those of Hernandez, Rumphius, Sloane, &c. The immense variety in the forms of plants was thenceforth recognized, and in point of taste the elegant simplicity of the primitive flowers was able to vie with the gaudiness of the double ones. Then ceased the reign of tulips and pæonies in flower gardens. Curiosity, that great incentive to all science, having penetrated horticulture, the change in gardens became rapid. Instead of a few hundred species such as were cultivated at the commencement of the last century, there are now 20,000 or 30,000 to be found in most of the present catalogues. The single family of Orchids has probably more different representatives in our hothouses than was the case with all the families of plants put together, a hundred years ago. Fashion, united to the present curiosity of amateurs, causes, from time to time, old plants to be abandoned for new ones; and thus the entire vegetable kingdom will ultimately pass under the observation of civilized man.

What would horticulturists do, amidst this invasion of thousands of species, had not botanists devised convenient plans of classification and nomenclature? The families, genera, and species, have all been arranged in books, just as the districts, streets, and numbers of the houses are in our great capitals—with this superiority of method, that the form of the objects indicates their place—as if, in looking at a house in a town, one might discover, at a glance, to what street and what quarter it belonged. The plan of giving a single name to each species, besides its generic name, together with the prohibition of changing names without due reason, of giving the same appellation to two different species or two genera, far excels our plan of distinguishing individuals. How much it would simplify our intercourse with men, and facilitate our inquiries, if, in the whole world, the members of one family only bore the same name, and if each individual had but one christian name, differing from those of the other members of his family. Such is, nevertheless, the admirable plan of nomenclature that science has provided for horticulturists, and which they cannot too much appreciate and respect.[19]

3. *The beneficial effects of the association of botany with horticulture.*—The pursuit of horticulture demands books and herbaria, as that of scientific botany requires cultivated living plants. Thence the necessity, which is more and more recognized, of bringing together the materials for comparison in the same town, the same establishment, and even under the same administration, organized so as to facilitate the use of them. How many institutions in Europe, either private or public, would be benefited by this arrangement! How many towns and countries are now deficient—some in libraries, some in herbaria, some in respect to horticulture. Professional men proffer their complaint; let us hope that public opinion may end by listening to them.[20]

The bringing together the means of study, I have said, is desirable. Not less so is the interchange of ideas and impressions, both of botanists and horticulturists. Each of these classes must clearly have distinct characteristics; but the one should be influenced by the other. By these means, some too retiring dispositions may be brought out, and certain dormant powers developed. Horticulture, for instance, has a commercial tendency which may be carried too far. Charlatanism may slide in among flowers. Botany, on the contrary, is a science, and consequently rests on the investigation of pure and simple truth. A horticulturist who allows himself to be influenced by a scientific spirit necessarily frees himself from over-selfish tendencies. Natural history, on its side, by reason of the perfection of its method, its nomenclature and its minute observations, has something technical and dry about it, which contrasts with the grandeur of nature, and with the sentiment of art. It is for horticulture, combining, as it does, the planning and the decorations of gardens, to develop the æsthetic faculties of the savant, as of the world in general. A lovely flower, beautiful trees, a splendid floral exhibition, excite a sort of admiration, and even enthusiasm, similar to the effects produced by music or painting.

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[19] Two years ago I made a request to the Fédération des Sociétés d'Horticulture Belges, which appears to have been favorably received, and it may not be useless to repeat it here. It consisted in begging the horticulturists who obtain new varieties not to give them botanical names, with a Latin designation, but merely arbitrary names of quite a different nature, in order to avoid confusion and useless researches in books. For example, if they called a Calceolaria, Sebastopol, or Triomphe de Gand, every one would understand it meant a garden variety; but if they named it Lindleyi, or mirabilis, one would think that it was a botanical species, and would search for it in scientific works, or in the Floras of Chili; and botanists, happening perhaps to mistake it, would add it to the end of a genus in their books as a species imperfectly known. The more horticultural names differ from Latin ones, the better it is, unless they can be appended to the scientific nomenclature: as when we say Brassica campestris oleifera, instead of, shortly, Colza.

[20] The Botanical Gardens at Kew are a fine example of what should be done, either on a large or a more modest scale, in many towns where the means of study are yet inconvenient or incomplete.

The powers of the German composers of modern days, and those of the Italian painters of the 16th century are justly extolled; but may it not also be said, that in point of art they are equalled in their way by the beautiful parks of old England? The feeling of harmony, in form and color—is it not also studied in them? The effect of contrast—is it not skillfully managed? The gradual transition from architectural to natural beauties—is it not treated in an admirable manner? Yes; decidedly the English landscape gardeners are poets; they have drawn from the same sources of inspiration as the most national writers of their country, and that source is the appreciation, so universal in England, of the beautiful, in an aspect of nature which is elegant and attractive, though somewhat grave.

Thus, gentlemen, for the development of our talents, as well as for our actual benefit, art and science keep pace together. Let us rejoice over their union, rendered conspicuous to-day by this congress of botanists, held in connection with a great floral exhibition; and after these general observations—perhaps rather too protracted—let us enter upon the consideration of those more truly scientific subjects, in which many among you are no doubt disposed to take part.

ART. XXXVII.—*Caricography*; by Prof. C. DEWRY.

Continued from vol. xli, p. 331.—1866. (The 43d No.)

No. 299. *Carex retrocurva*, Dew. 1845.

Spikes distinct; staminate single, terminal pedunculate cylindric; pistillate spikes 2–5, cylindric short-oblong rather close-flowered, often remote, leafy-bracteate long, and filiform-pedunculate pendulous or part resting on the earth, the lowest nearly radical; stigmas 3; fruit ovate or obtusish, short-rostrate, triquetrous, slightly nerved, about equaling its ovate cuspidate scale; culm 8–16 inches high, nearly erect, then subprostrate; leaves sub-radical, soft and wide; whole plant rather glaucous.

Open woods, Massachusetts and New York; south, north and west. When *C. oligocarpa* was confounded with *C. digitalis*, this was called by Dr. Gray *C. oligocarpa* var. *latifolia*, Gray, Gram. and Cyp., 1835, as quoted in Tor. Mon., p. 416, 1836.

No. 300. *C. stylosa*, Meyer. 1830.

Terminal spike erect, cylindric, short and staminate; pistillate spikes 2–3, often 2, short-cylindric, close-fruited, lowest leafy-bracteate and rather remote; stigmas 3; fruit ellipsoid triquetrous, stipitate, tapering above into a short beak with entire orifice, and exceeding the obtuse or ovate-oblong scale; culm scabrous above and leafy below; leaves linear, narrow and roughish on the margin.

Russian America—Unalaska, *Meyer*, and Sitka, *Bongard*. Contrary to the remark made in vol. xxix, p. 252, 1836, from Meyer's figure, this plant is not *C. Carltonia*, or even *C. Parryana*, Dew., but is far different. Mon. 1836.

No. 301. *C. Hartwegii*, Boott, Plant. *Hartwegianæ*. 1842.

Spike compound, 2–3 inches long; spikelets 5–7, oblong or short-cylindric, staminate at their summit, alternate, the upper approximate and sessile, the lowest sometimes compound, also pedunculate sub-remote and bracteate; stigmas 3; fruit oval or ovate-oblong, rostrate, sub-triquetrous bidentate, sub-scabrous on the margin above and nerved, a little exceeding its ovate or oblong lanceolate scale which is green on the pale back; culm 8 to 10 inches high, rather slender, not filiform, but leafy; leaves narrow, flat, often longer than the culm, slightly rough on the margin; plant pale green.

The *sterile, tumid and nerved scale between the spikelet and the axis at the base of the lateral spikelets*, noticed by Dr. Boott in this species and two others, is a very striking and curious character.

California, Dr. H. N. Bolander; Guatemala, *Hartweg*, says Dr. Boott in the above reference. From *C. Deweyana*, it differs in having *three* stigmas.

No. 302. *C. Davalliana*, Smith. 1800?

Flowers diœcious with an oblong simple spike, never androgynous; fruit *distigmatic*, oblong-lanceolate, rostrate and roundish, tapering above and commonly much recurved, sub-scabrous above, nerved and longer than the ovate acute or awned scale; culm 5–8 inches high; leaves short, radical and filiform; both somewhat scabrous.

Rocky Mountains, Richardson; common in northern Europe. Separated from *C. dioica*, L., by the fruit, and from *C. gynocrates*, Wormsk., by being truly diœcious, as that has stamens often on its fertile spikes, as well as wholly staminate spikes, and hence is described as androgynous and polygamo-diœcious. Fries, Lang and Anderson notice these differences. The plants from the Rocky Mountains are exactly like *C. Davalliana* from Europe, and President Smith of the Linnæan Society, is adequate authority for the specific name.

No. 303. *C. Gayana*, Desv.

Spike composed of 4–8 spikelets aggregated into an ovate head; spikelets staminate above, ovate, sessile, and the lower sometimes branched, or staminate and pistillate spikelets closely aggregated (Boott), or sometimes diœcious (Boott); stigmas two; fruit roundish ovate, short-acuminate or beaked, sub-scabrous above, shorter and narrower than the ovate-acuminate or broad ovate lanceolate or cuspidate scales; culm exceeding a foot in height and leafy toward the base; leaves narrow and long often as the culm, scabrous on the edge; all light green except the dark brown, and rusty-like spike.

Boundary Survey and Rocky Mountains, Fendler, 881, and Hall, of Ill.

Notes.—1. *C. fusiformis*, Chapman, in vol. vi, p. 244, 1848, has been cancelled by the author; but as it is an authentic form of *C. debilis*, it here receives the name, *C. debilis* var. *fusiformis*, Dew. It is described in the above reference.

2. *C. Oederi*, Ehrht., has occurred in a diœcious form; numerous pistillate spikelets on one culm, and the staminate one to three short spikes on another culm, but both growing from the same root; about fifteen inches high.

Grand Isle, Judge Clinton; a singular form.

3. *C. viridula*, Mx., is a var. of *C. Oederi*, Ehrht., as Dr. Torrey learned from an examination of the herbarium of Michaux, and as stated in this Journal, xxvii, p. 276, 1835, and Tor. Mon., p. 417, 1836. New England, the state of New York, and Canada have given forms of *C. Oederi*, *short, small, with smooth culm: spikes three, the two lower axillary or bracteate and pistillate entirely, nearly or quite sessile, and the upper one staminate below and all small, while the triquetrous rostrate acuminate fruit allies it to C. flava* and not to *C. triceps*. It differs enough from *C. Oederi* in Michaux to have another name, but not since *C. Oederi* has been found so variable a species, and yet so alike itself. It differs even more from *C. triceps* and *C. hirsuta*. So accurate was Dr. Torrey in that early day in the determination of nearly all the species of Carex given by Michaux.

4. *C. Buxbaumii*, Wahl., *C. polygana*, Schk., (not Muh.) has very variable spikes, stigmas 3 in United States, and culm sharply triangular and very scabrous to partially smooth. The pistillate scales in Wahl., are said to be *cuspidate, about equalling the fruit*, and by others to be ovate, mucronate or cuspidate. On specimens from Germany the scales generally agree with the description, though much longer on some, and on specimens from Rhode Island the same, as well as from Canada W.; from the Rocky Mountains, with obtuse scales much shorter or much longer than the fruit; from Kentucky, Michigan, and New York, with scales *ovate lanceolate or ovate cuspidate, often twice longer than the fruit*, and black or dark; rusty on the sides as usual; culm very stiff and rough. From the marsh, Bergen.

5. *C. striatula*, Mx., 1803; xxvii, 278, 1835.
— *blanda*, Dew.; x, 45, 1826.

These two were found to be the same by Dr. Torrey, as he had access to the herbarium of Michaux, and both are described under the last reference; of the latter, its synonym, *C. conoidea*, Muh., given, while some botanists placed it under *C. anceps*.

6. *C. vaginata*, Tausch, xli, p. 227, 1866, and var. *alticaulis*, Dew.

Both of these forms have been abundant this season in the marsh at Bergen. The former is the shorter and has the larger fruit; the latter has much the more slender culm with narrow leaves. The latter has also been collected in great numbers at Belleville, C. W., by Macoun, with few of the former. The *refracted* culm above the upper pistillate spike has been uncommon this year at either locality.

Art. XXXVIII.—*Mineral Notices;* by Charles Upham Shepard.

1. *On Hagemannite, a new mineral from Arksutfiord, Greenland.*

For my knowledge of the present species I am indebted to Mr. G. Hagemann, chemist to the Natrona chemical works, Alleghany county, Pennsylvania, for whom it is named and from whom I received it, along with its associates, pachnolite, cryolite, etc.

The mineral is in seams and veins of from one-third to half an inch in thickness, generally having white cryolite closely adhering to its sides, though it sometimes traverses a drusy ferruginous pachnolite. It has in some instances the appearance of having been deposited in layers over broad undulating surfaces, when it resembles certain opaline deposits, as menilite. Its color is ochre- or wax-yellow, rarely with a faint tinge of green; and being impalpable in structure, dull (or only faintly glimmering) and opake, it reminds one of a very compact iron-flint, or of the yellow variety of chloropal from Alar, Bavaria. Its streak is paler than its color. It is not difficultly frangible, and shows an even fracture. H.=3·0 to 3·5. G.=2·59 to 2·60. It adheres but feebly to the tongue, without emitting a strong argillaceous odor.

When held in the flame of a candle, it decrepitates with surprising energy, throwing considerable fragments to a distance. In order to prepare it for blowpipe experiments, the mineral requires to be heated in a closed tube, during which it is observed to evolve much water and hydrofluoric acid. The decrepitated fragments soften easily in the first heat of the blowpipe, but without assuming a globular shape like cryolite. The color of the fused fragment passes quickly from light-pearl to a dirty greenish-gray. To borax it only imparts a feeble iron tinge. Its powder heated in a porcelain crucible to full ignition lost 10·1 p. c., and fused into a pinkish-white hard mass with a very rough surface. Having satisfied myself of the homogeneous nature of the mineral and its leading constitution, I requested Mr. Hagemann to undertake its analysis; and I here subjoin the results he has reached.

"In the specimen selected, (G. =2·83) it was impossible to discover even with the microscope, anything but the yellow mineral, which showed in many places a crystalline structure, or minute golden yellow points. It was hard to pulverize. The powder was heated with sulphuric acid in a platinum dish, whereby, with slight warmth, it evolved $SiFl_2$. When farther heated, it turned white, was easily decomposed, and by boiling with water and chlorhydric acid gave a yellow solution. This, after filtering and oxydation by nitric acid, was precipitated with ammonia, much chlorid of ammonium having been previously added. The precipitate was redissolved and reprecipitated twice before it was free from lime. Lime was precipitated as usual, the filtrate evaporated to dryness and evaporated; the residuum redissolved and the soda and magnesia separated by means of acetate of baryta. Fluorine was determined by Wöhler's process, and silicium by boiling the mineral with carbonate of ammonia. The precipitated $\ddot{\overline{A}}l_2+\dddot{S}i$ was separated as usual. Water was determined by heating the mineral under lime. The mineral contains a trace of phosphoric acid.

	Al	Fe	Ca	Mg	Na	Fl	Si	HO	Insol.
	12·00	5·82	11·20	2·30	8·45	40·10	7·79	10·44	1·08
	12·21	5·87	11·16	....	...	40·51	....	....	....
	11·98	6·17	11·16	....	....	....	....	....	....
Mean,	12·06	5·96	11·18	2·30	8·45	40·30	7·79	10·44	1·08

$$\begin{array}{lll}
\mathrm{HO} = 10{\cdot}44\ \frac{10{\cdot}44}{9} = & 1{\cdot}16 & \text{or } 2\ \mathrm{HO} \\
\left.\begin{array}{l}\mathrm{Al} = 12{\cdot}06\ \frac{12{\cdot}06}{13{\cdot}6} = 0{\cdot}886 \\ \mathrm{Fe} = 5{\cdot}96\ \frac{5{\cdot}96}{28} = 0{\cdot}202\end{array}\right\} = & 1{\cdot}098 & \text{“}\ 2\left\{\begin{array}{l}\mathrm{Al}\\ \mathrm{Fe}\end{array}\right. \\
\mathrm{Ca} = 11{\cdot}18\ \frac{11{\cdot}18}{20} = & 0{\cdot}559 & \text{“}\ 1\ \mathrm{Ca} \\
\left.\begin{array}{l}\mathrm{Mg} = 2{\cdot}30\ \frac{2{\cdot}30}{12} = 0{\cdot}191 \\ \mathrm{Na} = 8{\cdot}45\ \frac{8{\cdot}45}{23} = 0{\cdot}367\end{array}\right\} = & 0{\cdot}558 & \text{“}\ 1\left\{\begin{array}{l}\mathrm{Mg}\\ \mathrm{Na}\end{array}\right. \\
\mathrm{Fl} = 40{\cdot}30\ \frac{40{\cdot}30}{19} = & 2{\cdot}125 & \text{“}\ 4\ \mathrm{Fl} \\
\mathrm{Si} = 7{\cdot}79\ \frac{7{\cdot}79}{14{\cdot}8} = & 0{\cdot}526 & \text{“}\ 1\ \mathrm{Si}
\end{array}$$

The deduction of a formula is difficult. The following is suggested:

$$2(\mathrm{Ca}+\tfrac{2}{3}\mathrm{Na}+\tfrac{1}{3}\mathrm{Mg})\mathrm{Fl}+(\tfrac{3}{4}\mathrm{Al}+\tfrac{1}{4}\mathrm{Fe})^2\mathrm{SiFl}^2+2\mathrm{HO}$$

but it is very complicated; and it is uncertain whether SiFl_2 is capable of combining thus with metals. The iron was found to be present as sesquioxyd."

2. *Cotunnite at South Hampton Lead Mine.*

In a recent number of this Journal I have described scheelite as a rare product of the Hampton lead mine. I am now able to add cotunnite (PbCl) as a production, though similarly scarce, of the same locality. Two or three specimens have been brought to me by one of my pupils (Mr. P. W. Lyman, of the Junior class in Amherst College); and I have since heard of a fourth specimen, found by another visitor of the mine. The crystals are small, and occur in groups lining druses of quartz. They have the form of right-rectangular prisms, are without transparency and perfectly milk-white. When reduced to a fine powder the mineral is soluble in water, from which the nitrate of silver throws down the chlorid of silver.

3. *Columbite at Northfield, Mass.*

This mineral was sent to me last autumn for determination by Mr. M. A. Brown, of Springfield, Mass., an enterprising mineralogist, now on his way to Montana. I visited the locality with him last month. It is on land belonging to Mr. Simeon Lyman, and situated about one mile northeast of the village. It occurs in a much disintegrated coarse-grained graphic granite, which here forms veins from ten to fifteen feet in width, traversing the micaceous schist. Beryl is also somewhat abundant in the vein, in crystals often several inches (up to ten) in diameter, of a pale greenish-white color, and generally peculiar by their uniform shortness or tabular form, and the regularity of their terminations by a single plane. In this respect they resemble the beryls of Goshen and Norwich. The columbite is tolerably crystallized, black and shining, with a specific gravity of 6·5, which, it will be observed, is much higher than that of the Connecticut localities and nearly identical with the Bodenmais variety. The largest fragments weighed only a few ounces; and the supply at the locality (which is mostly derived from the soil contiguous to the vein) is very limited.

Very interesting specimens of crystallized fibrilite in distinct white prisms having nearly the form of kyanite are occasionally met with in the drift of this region. The crystals penetrate a compact micaceous rock in all directions; and from their great hardness are found projecting at various angles quite beyond the surface, notwithstanding the attrition to which the masses have been subjected.

In the remote southeastern section of the town, on what is called Northfield mountain (on the road to Irving), the following minerals are frequent, viz., garnet, kyanite, epidote and beryl. Also at what has been called the Black-lead mine, on land of Mr. Piper, where a highly plumbagenous mica-slate has been a little worked with a view to plumbago, I noticed several interesting specimens of the astrophyllite variety of mica. Grace mountain in Warwick is visible to the northeast from this vicinity, and is the locality of the beautiful radiated black tourmaline associated with granular epidote.

4. *Spodumene in Winchester, New Hampshire.*

This is a continuation of the Goshen formation. The locality is on land of Mr. Brown (the father of Mr. M. A. Brown), a wheelright, whose house is just upon the line dividing Northfield from Winchester. The spodumene ledge comes into view directly adjoining his dwelling. The mineral does not exhibit distinct crystals, but only long bladed and easily cleavable masses, which form the bulk of the rock. It is chiefly interesting as our most northern locality of this species, and as a prolongation of the Goshen rocks.

ART. XXXIX.—*Brief Notices of several localities of Meteoric Iron;* by CHARLES UPHAM SHEPARD.

1. *Savisavik,*[1] *North Greenland.*

THIS meteoric iron has been in my possession upward of two years; and I had hoped, before describing its locality, to have obtained a supply of material fully adequate to its description and analysis; but not succeeding in this I deem it best to delay my notice of it no longer.

My specimens, consisting merely of a few scales, scarcely larger than one's finger nail, were the gift of John C. Trautwine, Esq., Civil Engineer, of Philadelphia, to whom they had been presented by Dr. Hays, the well-known arctic voyager, accompanied by the following note:

"* * I send you the fragment of iron (supposed to be meteoric) which I promised. It was obtained from an Esquimaux at my winter station of Port Foul, in 1861, who had obtained it at a place called Savisavik, and had carefully preserved it with a few other fragments, to make (with an ivory blade) the edge of a knife. The name of the place is derived from "savik," *knife*, and means the place where knife-material is found, i. e., iron place. The Esquimaux told me that there is a large mass of this material, and that the natives go there frequently to obtain it. The locality is about twenty miles south and east of Cape York, North Greenland, near latitude 76°. The Esquimaux scale off fragments with flint stone.

Yours, etc., I. I. HAYS."

Philadelphia, April 17, 1864.

This iron is perfectly malleable and remarkably homogeneous, without being much prone to oxydation. Its specific gravity is just below 8·, which is rather high, but doubtless occasioned by the condensation it has suffered in being detached from the parent mass. For a time it was supposed that these fragments had been brought from the Niakornak locality in North Greenland; but a comparison of specimens fully disproves the idea. Some collectors have expressed doubts moreover, whether the last mentioned locality was a genuine meteorite. On this point, however, I have entertained no doubt, and am happy in being supported in this opinion by so high an authority as that of the late Prof. Forchhammer, who, in a letter to me dated Copenhagen, March 8, 1865, observes, "the Greenland (Niakornak) meteoric iron is certainly no artifical product, although it contains but little nickel and cobalt, and so much carbon, that it is hard and brittle like cast-iron.

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[1] It is not plain from Dr. Hays's letter whether this word begins with S or G.

2. *Botetourt County, Virginia.*

This iron was discovered more than fifteen years ago in a mass so ponderous that the finder, having attempted to transport it on horseback a number of miles to his house, was obliged to abandon the undertaking. He left it upon a stone wall by the road-side, after having (with the assistance of a negro who happened at the time to be passing with a hammer,) detached two or three small angular fragments. These were afterward given to Mr. N. S. Manross, who took them with him to Göttingen, where in the laboratory of Prof. Wöhler he analyzed one of them so far as to determine the presence of nickel in the very unusually high proportion of more than 20 per cent. In the year 1860, while Mr. Manross was delivering lectures in this college on chemistry, he presented me a little fragment of this iron along with the foregoing information; and after his melancholy death at the battle of Antietam, his widow gave me the only remaining specimen of it that is known.

The quantity is too small to justify a further analysis; and I content myself with a brief description of its physical properties. It is whiter than most irons, extremely close and homogeneous, with exception of a few minute pyritic grains. Specific gravity =7·64. Fracture fine granular like cast-steel. It does not give the Widmannstätian figures. In composition and structure it resembles the Green county (Tenn.) iron. As it is a variety not prone to decomposition, it is to be hoped that the original mass may yet be rediscovered, although it is scarcely probable that either of the persons who were once acquainted with its position are still alive.

3. *Colorado.*

If neither of the two preceding irons are likely to be represented in our collections, there is certainly a prospect that it will be quite otherwise with the mass just discovered upon the eastern slope of the Sierre Madre Range of the Rocky Mountains.

For my acquaintance with this discovery I am indebted to the kindness of Mr. J. Alden Smith, a practical mineralogist, at present residing in Colorado. This gentleman has transmitted to me by mail a very interesting cleavage lamina, 1½ inches long by ⅝ths of an inch wide and ⅛th thick, and which shows on one edge a portion of the natural coating of the meteorite. His letter, dated June 21st is very brief, though it contains important particulars which I cannot withhold from the scientific public until his return to the east in the coming autumn. By means of the promised specimens he expects to bring with him on his return, I hope to be able to give a more circumstantial account of the discovery.

The detection of the mass, and which has occurred only within a few weeks, is due to Messrs. Wilson and Morrison, by whom Mr. Smith was shown to the locality. It is situated within a very deep ravine, at the elevation of 8000 feet above the ocean and surrounded with high mountains on all sides. The exact dimensions of the mass are not given; but its weight is supposed to be several hundred pounds. "It seems to have struck a crevice in the solid ledge, and thereby to have been much shattered at one extremity,—a circumstance that enabled the finders to detach several small pieces." They inferred the fall to have taken place at a very remote period, as the mass exhibited a coating of oxyds half an inch thick. "Its composition is principally the native metals, iron, nickel, cobalt, a little manganese and a trace of copper. In some parts, iron forms the chief ingredient, while in others nickel and cobalt are largely in excess."

The specimen in my possession exceeds every iron I have seen in the perfection of its crystallization. It is as coarsely crystalline as that of Arva (Hungary) or Cocke county (Tenn.), but much more intimately laminated with Schreibersite than either. The laminæ of this substance are unusually thick, and possess a light color together with a bright luster. As they are disposed in accordance with the octahedral cleavage of the iron, they render the Widmannstätian figures strikingly apparent without polishing or the use of acids. No pyrites or graphite is visible in my specimen. Specific gravity =7·43.

4. *Supposed new locality in Tennessee.*

Through the kindness of a scientific friend in Mississippi, Dr. W. Spillman, I am able to announce the very recent discovery of a considerable mass of meteoric iron upon a mountain in Tennessee. It was accidentally met with by a mining explorer, and is described as being "as large as a man can lift." The finder hammered off only a fragment of very small size which was forwarded to me by letter. Its original structure had been destroyed by the process of detaching the fragment. It was nevertheless highly malleable with the usual luster and color of meteoric iron. It was rapidly dissolved by chlorhydric acid without the odor of sulphuretted hydrogen. After being treated by nitric acid and ammonia, the characteristic purplish blue of the ammoniacal solution of $\dot{\mathrm{N}}\mathrm{i}$ was exhibited. Measures have been taken to secure the mass, when a full description of it will be given.

Amherst College, July 9, 1866.

ART. XL.—*Appendix to Article XXX, On the Origin of some of the Earth's Features;* by JAMES D. DANA.

ON page 210 I have made but a bare allusion to the question of the heat required in metamorphism. Mr. Vose dispenses with heat altogether, except what may be incidental to compression. And Professor Hall regards it as of secondary importance, or not absolutely necessary (see our citation on page 207), and attributes the little extraneous heat that may be present and operative—probably, he says "not much above that of boiling water" (Pal., vol. iii, p. 77)—to the sinking of the thickening deposits to a level "where the surrounding temperature was higher;" higher, that is, on the principle, first suggested by Herschel, of the rising of the isothermal planes within the earth's crust in concordance with increase of thickness through superficial deposits; the isothermal plane of 100°, for example, being within a certain distance of the surface of the crust in a given region, and rising as the surface rises by new accumulations above.

The correctness of Herschel's principle cannot be doubted. But the question of its actual agency in ordinary metamorphism must be decided by an appeal to facts; and on this point I would here present a few facts for consideration.

The numbers and boldness of the flexures in the rocks of most metamorphic regions have always seemed to me to bear against the view that the heat causing the change had ascended by the very quiet method recognized in this theory. For the heat, thus slowly creeping upward, a few inches, feet, or yards in a century, should produce the change with little disturbance in the mass, and leave the beds nearly or quite horizontal: a condition very unlike that actually found in nature. The region of the thickened accumulations is also necessarily, as I have said, one of *strengthened* crust, under the gravity-hypothesis; and displacements, from any expansion of the crust which the slowly ascending heat might produce, should be mainly apparent in the surrounding regions where the crust had not been thus thickened.

But there are other facts indicating a limited sufficiency to this means of metamorphism. These are afforded by the great faults and sections of strata open to examination. In the Appalachian region, both of Virginia and Pennsylvania, faults occur, as described by the Professors Rogers, and by Mr. J. P. Lesley, which afford us important data for conclusions. Mr. Lesley, an excellent geologist and geological observer, who has explored personally the regions referred to, states that at the great fault of Juniata and Blair Cos., Pennsylvania, the rocks of the Trenton period are brought up to a level with those of the Chemung, making a dislocation of at least 16,000, and probably of 20,000, feet. And yet the Trenton limestone and Hudson River shales are not metamorphic. Some local cases of alteration occur there, including patches of roofing slate; but the greater part of the shales are no harder than the ordinary shales of the Pennsylvania Coal formation.[1]

At a depth of 16,000 feet the temperature of the earth's crust, allowing an increase of 1° F. for 60 feet of descent, would be about 330° F.; or with 1° F. for 50 feet, about 380° F.—either of which temperatures is far above the boiling point of water; and with the thinner crust of Paleozoic time the temperature at this depth should have been still higher. But, notwithstanding this heat, and also the compression from so great an overlying mass, the limestones and shales are not crystalline. The change of parts of the shale to roofing slate is no evidence in favor of the efficiency of the alleged cause; for such a cause should act uniformly over great areas.

In Southern Virginia, between Walker's Mountain and the Peak Hills, the Trenton rocks, as Lesley observes, are brought up, by means of a fault, to a level with the Lower Carboniferous. The amount of the fault by the lowest estimate is 15,000 feet. Notwithstanding the depth at which the Trenton beds had been lying previous to the faulting, the limestones are not granular marbles, but ordinary stratified limestone.

Again, in the great Nova Scotia section, at the Joggins, 15,000 feet of rock are exposed to view out of the 16,000 feet or more of the whole Carboniferous formation; and the lower strata of these 15,000 feet consist of shales and sandstones, and *fossiliferous* limestone, without metamorphism.

What is the natural inference from these data? Can we assume that the Coal formation in Rhode Island and Massachusetts, now in a high state of metamorphism, or the Devonian rocks of New England, now granite, gneiss, crystalline schists and marble, were once covered with deposits standing 15,000 feet above the present surface? As daring as this assumption would be, the condition would not give, as the facts show, the heat requisite for the metamorphism that has taken place.

We may say further: The pressure (Hall), or compression (Vose), from 15,000 feet of overlying rock, enormous though it be, added to the heat derived from below on the principle explained, and to the tension from expansion through this heat, and to any movements resulting from this expansion, is not sufficient to produce distinct metamorphic changes.

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[1] In a recent conversation with Mr. Lesley, he confirmed these statements, and said that the upturned rocks are so situated that the approximate thickness of the series is easily ascertained. The facts are briefly alluded to in my Manual of Geology, on page 707.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the chlorids af tungsten.*—DEBRAY has studied the vapor-densities of the volatile compounds of tungsten with chlorine, and with chlorine and oxygen, and has arrived at results of much theoretical interest. When a current of dry chlorine is passed over metallic tungsten heated to redness in a tube of hard glass, red vapors are obtained which condense to a dark gray mass, which is a mixture of the two chlorids WCl_3 and W_2Cl_5. By distillation in a current of chlorine the terchlorid WCl_3, may be obtained very nearly pure. There are, as is well known, two oxychlorids of tungsten corresponding to tungstic acid, the formulas of which are respectively WO_2Cl and $WOCl_2$. Debray obtains these bodies easily by Gerhardt's method by distilling the terchlorid with dry oxalic acid. The red oxychlorid or, as we should prefer to term it, oxydichlorid, $WOCl_2$, is easily obtained pure, but the yellow or dioxychlorid, WO_2Cl, is always mixed with tungstic acid or the oxydichlorid, as it is easily decomposed by heating, according to the equation

$$2WO_2Cl = WOCl_2 + WO_3.$$

The terchlorid heated with tungstic acid acts upon it with evolution of heat, according to the equation,

$$WO_3 + 2WCl_3 = 3WOCl_2.$$

The easy decomposition of the dioxychlorid makes it impossible to determine the density of its vapor, but the vapor-densities of the two other compounds may be readily taken in the vapor of mercury or sulphur, since the least volatile boils at about 300° C. In this manner the terchlorid gave in the vapor of mercury the density 11·50, and in the vapor of sulphur 11·89, 11·80 and 11·69. The oxydichlorid gave in the vapor of mercury 10·78 and 10·70, and in the vapor of sulphur 10·27.

If we admit that the formulas of these bodies correspond to 2 or 4 vols. of vapor, we find by calculation that the theoretical density of the terchlorid is 13·75 on the hypothesis of 2 vols., and 6·875 on the hypothesis of 4 vols., and that the vapor-density of the oxydichlorid is 11·86 on the hypothesis of 2 vols., and 5·93 upon that of 4 vols. If we admit with Persoz that tungstic acid is $W'O_5$ and the perchlorid $W'Cl_5$, the equivalent of the chlorid becomes five-thirds of the old equivalent and its vapor-density five-thirds of that found above theoretically, so that if $W'Cl_5$ corresponds to 4 vols. of vapor we have for its vapor-density 11·46, which agrees nearly with that found by experiment. We must then suppose, however, that the oxydichlorid is $WO_{\frac{5}{3}}Cl_{\frac{10}{3}}$, when the calculated density upon the hypothesis of 4 vols. becomes 9·87. And as fractions of equivalents cannot be admitted we must write this formula $W'O_5 + 2W'Cl_5$, which is the same as admitting the existence of bodies the vapor-density of which corresponds to 12 volumes.—*Comptes Rendus*, lx, 820. W. G.

2. *On the separation of cobalt from nickel.*—TERREIL has given a method of separating cobalt from nickel which promises to yield good results. To the solution containing the two metals ammonia is to be

added in excess so as to dissolve the oxyds. To the hot liquid a solution of hypermanganate of potash is to be added until present in excess, as shown by the violet color of the solution remaining for a short time. The whole is then to be heated to boiling for some minutes and then a slight excess of chlorhydric acid added so as to dissolve the precipitated oxyd of manganese. The liquid is then to be kept for twenty or twenty-five minutes at a gentle heat and allowed to stand for twenty-four hours. All the cobalt is then deposited in the form of a crystalline powder of a beautiful reddish violet color (chlorid of purpureocobalt), which is to be thrown upon a weighed filter, and washed in the cold at first with dilute chlorhydric acid or a solution of sal-ammoniac, and afterward with alcohol. The filter and salt may then be dried at 110° C. and weighed. Or a known weight of the salt may be reduced in a current of dry hydrogen when pure cobalt will remain. The filtrate containing the nickel is to be boiled to expel alcohol, supersaturated with ammonia and hypermanganate of potash added. On boiling the whole of the manganese will be thrown down, while the filtrate contains the nickel. An alkaline hypochlorite may be used in place of the hypermanganate to oxydize the cobalt, but in this case the deposition of the chlorid of purpureocobalt requires several days to be completed.—*Bull. de la Soc. Chimique de Paris*, Feb. 1866, p. 88. W. G.

3. *On a new alcohol in which carbon is partially replaced by silicon.*—FRIEDEL and CRAFTS have succeeded in replacing carbon by silicon by a somewhat circuitous process of the greatest interest in a theoretical point of view. Chlorine acts upon silicium-ethyl, $\mathrm{Si}(\text{Ꞓ}_2\mathrm{H}_5)_4$, to form two products of substitution, monochlorinated and dichlorinated silicium-ethyl. These products cannot be separated by distillation, but when the mixture of the two, boiling between 180° C. and 200° C., is heated in a closed tube with acetate of potash and alcohol, the bichlorinated compound is first attacked, while chlorid of potassium is formed and the monochlorinated compound remains among the products of the action. When water is added to the contents of the tube after the action, an oily liquid separates which is to be washed twice with water and then treated with concentrated sulphuric acid, which dissolves the acetic acid compound and the oxyd of silicium-triethyl, $\left.\begin{matrix}\mathrm{Si}(\text{Ꞓ}_2\mathrm{H}_5)_3\\ \mathrm{Si}(\text{Ꞓ}_2\mathrm{H}_5)_3\end{matrix}\right\}\Theta$, leaving the silicium-ethyl and its chlorine derivatives unacted upon. The portion undissolved is to be washed, dried and distilled. The greater part passes over at 180°–190°, and is treated as before in a closed tube with acetate of potash and alcohol. The liquid separated by water is again treated with sulphuric acid, the solution decanted and poured into water. A liquid separates which boils between 208° and 214° C., has a faint ethereal and acetic smell, and burns with a luminous flame, giving off white fumes of silicic acid. This liquid has the formula $\left.\begin{matrix}\mathrm{Si}\text{Ꞓ}_8\mathrm{H}_{19}\\ \text{Ꞓ}_2\mathrm{H}_3\Theta\end{matrix}\right\}\Theta$, and is derived from monochlorinated silicium-ethyl by replacing the chlorine by oxacetyl, $\text{Ꞓ}_2\mathrm{H}_3\Theta$. Treated with an alcoholic solution of caustic potash this body yields a new liquid boiling at 190° C., and having the formula $\left.\begin{matrix}\mathrm{Si}\text{Ꞓ}_8\mathrm{H}_{19}\\ \mathrm{H}\end{matrix}\right\}\Theta$, which is the hydrate corresponding to the acetate above

described. The authors term the radical, $\text{SiЄ}_8\text{H}_{19}$, *siliconoyl*, and compare the hydrate and acetate to the corresponding compounds of carbon and hydrogen, $\left.\begin{matrix}\text{Є}_9\text{H}_{19}\\ \text{H}\end{matrix}\right\}$ and $\left.\begin{matrix}\text{Є}_9\text{H}_{19}\\ \text{Є}_2\text{H}_3\Theta\end{matrix}\right\}\Theta$, considering silicium to replace carbon atom for atom.—*Comptes Rendus*, lxi, 792. W. G.

4. *On a new class of organic radicals containing metals.*—**Berthelot** has described a very remarkable series of bodies derived from acetylene, C_4H_2 or $\text{Є}_2\text{H}_2$, and containing copper, silver, &c., in place of the hydrogen of the primitive radical. The first of these bodies, *Cuprosacetyl*, has the formula $\text{C}_4\text{Єu}_2\text{H}$; its oxyd, $(\text{C}_4\text{Єu}_2\text{H})\text{O}$, is obtained by precipitating the ammoniacal subchlorid of copper by acetylene and washing the precipitate by decantation with strong ammonia. The oxyd is a reddish brown flocky precipitate, decomposed by chlorhydric acid with formation of subchlorid of copper and acetylene. It decomposes sal-ammoniac solution when boiled with it, and is itself decomposed with difficulty by boiling with sulphurous and sulphuric acids.

The chlorid of cuprosacetyl is obtained by passing acetylene, bubble by bubble, into a concentrated solution of subchlorid of copper in chlorid of potassium. The gas is absorbed and a yellow precipitate is formed, which soon becomes crystalline. This precipitate, washed by decantation with a saturated solution of chlorid of potassium, becomes orange, then purple, and finally dark red. The precipitate is finally washed with water, when the chlorid remains pure. It is decomposed by ammonia with formation of the oxyd, and by boiling chlorhydric and nitric acids. It unites with chlorid of ammonium to form a double salt of a darker color than the corresponding potassium compound. The bromid and iodid of cuprosacetyl may be obtained by similar methods. The iodid has a magnificent vermillion-red color, and is much more stable than the chlorid and bromid. It closely resembles iodid of mercury but is insoluble in iodid of potassium. It forms an orange-yellow double salt with iodid of potassium, and also an oxyiodid. The author has also prepared a yellow oxycyanid and a basic sulphite of cuprosacetyl. The sulphid is obtained by the action of sulphuretted hydrogen water upon the oxyd, and is mixed with subsulphid of copper. Allylene gives similar compounds with the subsalts of copper; the chlorid and iodid are yellow.

The argentic compounds of acetylene are analogous to those of copper. They may be deduced from the radical $\text{C}_4\text{Ag}_2\text{H}$, which the author terms argentacetyl. The oxyd of this radical may be obtained by treating acetylene with nitrate or any oxysalt of silver dissolved in ammonia, and washing the precipitate first with ammonia and then with distilled water. It is the compound already known as acetylid of silver, and has the formula $(\text{C}_4\text{Ag}_2\text{H})\text{O}$. The chlorid of this radical is a white curdy precipitate resembling chlorid of silver. The sulphate is a grayish-white substance decomposed by chlorhydric acid. The phosphate is a yellow curdy precipitate. Berthelot remarks that these compounds are the first known organic radicals containing copper or silver, and that in their mode of formation they differ from the radical resembling the ammonia-metallic bases of Gros, Reiset, &c., in being formed by the direct action of a hydruret upon a metallic salt. The analogy between cuprosacetyl and ammonia may be shown by the following formulas:

NH_3	C_4H_2	$C_4Ȼu_2$		$C_4ȻuH$	C_4AgH
NH_4	$C_4H_2.H$	$C_4Ȼu_2.H$	or	$C_4ȻuH.Ȼu$	C_4Ag_2H
NH_4O	....	$C_4Ȼu_2HO$	or	$C_4ȻuH.ȻuO$	C_4Ag_2HO

The author further remarks that $(C_4ȻuH.Ȼu)O$ and $(C_4AgH.Ag)O$ are analogous to the oxyd of Reiset's base, $(NH_3Pt)O$, and that various facts lead him to believe that there are compounds analogous to the base $(2NH_3Pt)O$, such as $[(C_4AgH)_2Ag]O$.

In a subsequent paper the author describes similar compounds containing gold and chromium, the constitution of which, however, is not yet clearly ascertained. Silver unites with allylene to form argentallyl, the chlorid of which has the formula $[C_6H_3Ag(C_6H_3Ag_2)]Cl$, so that the radical corresponds to the second series of acetylene compounds above mentioned. When metallic sodium is heated in acetylene the gas is readily absorbed and a compound is formed having the formula C_4HNa, while the hydrogen set free unites with another portion of acetylene and forms ethylene, C_4H_4, and its hydruret, C_4H_6. Potassium acts in a similar manner but with more violence. At a higher temperature sodium replaces all the hydrogen and form C_4Na_2. The results given are to be considered as preliminary to a fuller investigation of the subjects.—*Bulletin de la Société Chimique*, March, 1866, pp. 176, 182. W. G.

5. *Isomerism.*—Berthelot, in a memoir on a new kind of isomerism, proposes the following subdivision of this subject. Isomeric bodies—that is to say, bodies formed of the same elements united in the same proportions—can be separated into a certain number of classes or general groups:—

(1.) *Equivalent composition.*—Substances which appear to have a purely accidental relation to each other; for instance, butyric acid $C_8\,H_8\,O_4$ and dialdehyde $(C_4\,H_4\,O_2)_2$.

(2.) *Metamerism.*—Bodies formed by the union of two distinct principles, so that in their formulæ a kind of compensation is established; for example, methylacetic ether, $C_2\,H_2\,(C_4\,H_4\,O_4)$ and ethylformic ether, $C_4\,H_4\,(C_2\,H_2\,O_4)$.

(3.) *Polymerism.*—Compounds arising from the union of several molecules to form one; this is shown in the case of amylene $(C_{10}\,H_{10})$ and diamylene $(C_{10}\,H_{10})_2$.

(4.) *Isomerism, properly so-called.*—There are bodies that, differing in their properties, retain these distinctive features in their passage through certain compounds, the properties of which result from the internal structure of the compound molecule taken as a whole, rather than the diversity of the components which have produced it. This is observed in the cases of essence of terebenthine and citron, the sugars, the symmetrical tartaric acids, and the two classes of ethyl-sulphates.

(5.) *Physical Isomerism.*—By which is meant the different states of one and the same body, the diverse nature of which vanishes when the substance enters into combination. To these five classes, Berthelot proposes to append a new one, called kenomerism (from *κενὸν*), distinct from all the others, though allied to metamerism.

(6.) *Kenomerism.*—Two different compounds may lose, by the effect of certain reagents which bring about decomposition, different groups of elements, and the remainders be identical in composition; these two derivatives, however, may yet be distinct the one from the other both in physical and chemical properties. They retain to some extent the structure of the compounds from which they take their origin. To take examples: alcohol by losing 2 equivalents of hydrogen is turned into aldehyde: $C_4 H_6 O_2 - H_2 = C_4 H_4 O_2$.

Glycol, on the other hand, by giving up 2 equivalents of water, is converted into glycolic ether (oxyd of ethylene):

$$C_4 H_6 O_4 - H_2 O_2 = C_4 H_4 O_2.$$

Glycolic ether and aldehyde are isomeric; their composition is the same, but their properties, both physical and chemical, are extremely different. This is a good case of kenomerism. Again, essence of terebenthine combines with hydrochloric acid under different conditions to form *two* distinct hydrochlorates, the monohydrochlorate, $C_{20} H_{16} HCl$, and the dihydrochlorate, $C_{20} H_{16} 2HCl$. From the first body the crystalline compound $C_{20} H_{16}$, camphene, is obtained, and from the latter $C_{20} H_{16}$, terpilene, two hydrocarbons of very different properties.—*Reader*, July 7.

6. *On a new determination of the velocity of sound in different media.* —August Kundt has, by a course of experimental investigations performed in the laboratory of Magnus of Berlin, arrived at new and very interesting results in regard to the longitudinal vibrations of gases, and disclosed a new method for the determination of the velocity of sound in gases and solids, which gives as accurate results as any other method, and besides is admirably adapted for the class-room.

After having enlarged our knowledge of longitudinal vibrations of glass tubes coated on the inside with lycopodium, Mr. Kundt closed one or both ends of the longitudinally vibrating glass tube; instead of the accumulations observed by Savart he found the lycopodium to form a beautiful regular wave-line with transverse ripplings, varying according to definite variations in the circumstances of the experiment.

Take a glass tube about four feet long and three-fourths of an inch in diameter, shake some lycopodium into the same so as to make it adhere like dust to the walls of the tube, close each end by a cork, hold the tube in the middle, and cause it to vibrate longitudinally; then there will always be 16 heaps of the lycopodium. The velocity of sound in glass being about 16 times as great as in the air, in the tube the distances between the heaps, produced by the stationary waves are corresponding parts of the wave-length of the tone in glass and air (here one-half wave-length). This number is therefore found to be independent of the dimensions of the glass-tube; Kundt has used tubes of from one foot long and one-twelfth of an inch diameter, to six feet long and three inches diameter. If the glass tubes vibrate with two nodes, there are always 32 heaps; with three nodes there are 48 heaps,—the distance between the heaps being always one-half wave-length for air; but the glass tubes when held in the middle give one-half wave-lengths, that is, when vibrating with two nodes one wave in glass, when with three nodes $\frac{3}{2}$ waves in glass, thus giving $\frac{1}{2} : 16 = 1 : 32 = \frac{3}{2} : 48$.

When the tube is held in the same manner, that is, when its length is the same part of a glass-wave, *the distance of the heaps* (half-wave lengths in the gas) *will be proportional to the velocity of sound in the gas*, or the number of heaps will be inversely proportional to that velocity.

For tubes filled respectively with air, carbonic acid, illuminating gas and hydrogen, Mr. Kundt obtained respectively 32, 40, 20, and 9 heaps, from which the velocity of sound (air $= 1$) is for carbonic acid $\frac{32}{40} = {\cdot}8$, illuminating gas $\frac{32}{20} = 1{\cdot}6$, hydrogen $\frac{32}{9} = 3{\cdot}56$. Dulong found, by a very difficult method, for carbonic acid ·79, for hydrogen 3·8

To obtain still greater accuracy, and also determine the velocity of sound in different solids, Kundt closes one end of the glass tube by a cork, movable by means of a wire; while the other end is closed by a perforated cork, enclosing a rod of the solid submitted to the experiment. This solid rod has one-half of its length in the glass tube, which itself is somewhat longer than the entire rod. This rod is set in vibration.

It will easily be seen, that for the same mode of vibration the velocity of sound in the solid will be directly proportional to the length of the rod, and inversely proportional to the distance of the lycopodium heaps in the glass tube.

With a brass rod 941·5 mm. long and 5 mm. diameter, Mr. Kundt obtained, in three different experiments, in each making numerous measurements of the distances, the velocities 10·87, 10·87, 10·86. Another brass rod gave 10·94 and 10·90. Similarly for steel, 15·345, 15·334 and 15·343; for glass, 15·24, 15·25 and 15·24; for copper, 11·960.

Wertheim found for cast-steel, 14·961; for steel wire, 15,108; for copper, 11·167.

The above leaves no doubt that Mr. Kundt has enriched science with a *new method* for the determination of the velocity of sound in solids, gases and vapors, alike excellent for a high degree of accuracy in its numerical determinations, ease of execution, elegance and simplicity, making it exceedingly convenient for lecture experiments.

We are engaged in experiments to try the application of this method to liquids.—*Poggendorff's Annalen*, 1866, cxxvii, 497–523; *l'Institut*, 1866, p. 15–16; *Cosmos*, 1866, iii, 98–100. G. H.

7. *The vapor of water not absorbent of much radiant heat.*—TYNDALL and FRANKLAND have, on the basis of some experiments, ascribed to watery vapor an excessive absorptive power for heat. The former even says: "Comparing a single molecule of aqueous vapor with an atom of ether of the main constituent of our atmosphere, I am not prepared to say how many thousand times the action of the former exceeds that of the latter." (Lecture on Radiation, Sect. 12.)

Magnus has objected to these experiments because they did not insure the absence of *condensed* vapors; he has now succeeded in constructing an apparatus which affords positive proof of the presence or absence of condensed vapor, "fog." He has found that the *radiation* (which is proportional to the absorption) of the following gases and vapors gave the following deflections with his very delicate thermo-multiplier, all the gases being heated about to 230° C.: dry atmospheric air 3 mm.; air having passed through water 3 to 5; dry carbonic acid gas 100 to 120; common illuminating gas, about the same; air having passed through boiling water, irregular, but maximum deflection only 20, and only gradually increasing to this amount, while carbonic acid and illuminating gas produced the deflection *suddenly*. When the water boiled so strongly that fog became visible at the radiating point, the deflection was above 100.

From the circumstances attending the deflection of 20 mm., even this may be ascribed to the presence of fog.

Magnus, as well as Dove, Quincke, Riess, Kundt, and others who witnessed the experiments, have seen that air passed through water at the common temperature never gave a greater deflection than 3 mm.; when saturated at a higher temperature never greater than 20 mm.; and only when fogs appeared the deflection became about as great as with carbonic acid gas, viz., 100 mm.

Magnus also experimented with a number of other vapors. He also shows how the phenomena of *dew* are in accordance with his view; that dew would be impossible if watery vapor had so great an absorptive power as Tyndall supposes; but that all the deductions of Tyndall and Frankland in regard to climate and the glacial period remain true if we substitute fog or foggy vapor for true uncondensed vapor; and finally, that the aqueous absorption lines in the spectrum observed by Cooke and Secchi are contradictory to any extraordinary absorptive power in actual vapor.—*Poggendorff's Annalen*, 1866, cxxvii, 613–624.

G. H.

8. *Solar spots influenced by solar refraction.*—In a certain sense the observations of Carrington (this Journal, xxxviii, 142) and of Spörer have thrown the subject of the physical constitution of the sun back into uncertainty and doubt. But it seems that as little as Kirchhoff's observations upset our views of the constitution of the Laterna mundi of Copernicus, so also the remarkable observations above referred to seem rather destined to confirm than to destroy the more ancient hypothesis of several atmospheres of the sun; for Mr. Dauge, of the Academy of Brussels, has shown how all the striking phenomena observed by Carrington and Spörer may be fully accounted for by the refraction of the emergent rays in the atmosphere exterior to the photosphere of the sun. By a very simple elementary process Mr. Dauge demonstrates that such an atmosphere by its refraction will produce the following effects: 1st, augment the apparent diameter of the sun; 2d, augment the mean period of rotation of the sun; 3d, retard the apparent motion of a spot in proportion as the same recedes from the center toward the rim of the sun; 4th, the apparent period of revolution of a spot increases with its solar latitude; 5th, the solar refraction produces an apparent motion of the spots in latitude (the latitude decreasing from the eastern rim to the middle, and increasing from the middle to the western limb of the sun).

Taking the horizontal refraction of the exterior atmosphere less the apparent diameter of the sun at 25°, and neglecting some insignificant terms, Mr. Dauge obtains the following value of the period of revolution of a spot at a solar latitude λ

$$4\tau' = \overset{\text{days}}{25{\cdot}30} \times \frac{90° + \beta}{115°}$$

where $\beta = \frac{\sin 25°}{\cos \lambda}$.

Taking the mean of Carrington's observations for every fifth degree of latitude, Dauge gives the following comparison between the observed (O) and calculated (C) values of the period of revolution expressed in days:

λ	0°	5°	10°	15°	20°	25°	30°	35°	47½°
O	25·30	25·11	25·20	25·51	25·73	25·90	26·34	26·92	27·96
C	25·30	25·33	25·38	25·50	25·68	25·92	26·22	26·80	28·33

Toward 45° the observations are very scarce, hence of little weight.

The agreement of these numbers is sufficient to prove that this refraction is the principal cause of the phenomena observed by Carrington and Spörer. It may be well to remember that Secchi had suggested the influence of solar refraction some time before the publication of Dauge's work.—*L'Institut,* 1866, pp. 159, 165–168. G. H.

II. MINERALOGY AND GEOLOGY.

1. *Geological explorations in Northern Mexico;* by A. Rémond. Compiled from his notes, and prepared for publication, by J. D. Whitney. 18 pp. 8vo. San Francisco, 1866.—We cite a few paragraphs from this valuable report on the geology of Northern Mexico.

"The mountainous region comprising the central and western portion of Northern Mexico belongs to the four states of Durango, Chihuahua, Sinaloa, and Sonora. Considering how celebrated this portion of Mexico has become for its mines and metalliferous veins, and how much has been written about it, it is surprising how little exact information has hitherto been obtained with regard to either its geography or geology. On comparing the principal published maps of the region in question, it will be seen at once how much they differ from each other in their delineations of even its main topographical features, while the details are entirely wanting.

"The name of the 'Sierra Madre' is usually applied to the main range of mountains of this country, or the western border of the plateau which stretches north through the territories of the United States, forming what may be called the great orographical feature of the continent. In northwestern Mexico this crumpled border of the great plateau comprises an extensive mountainous region, by no means forming a continuous single chain, but rather several central ranges, with associated groups of parallel ridges, all having the same general course, which is approximately north-northwest, and south-southeast. As the breadth of the chain widens as we go toward the north, so, too, that of the valleys increases in that direction, the whole system of mountains and valleys spreading out in something like a fan shape.

"Going north, the chain appears to sink gradually, although determinations of altitude in northern Mexico are extremely few in number. It is certain that there is, in about latitude 32°, a depression of the mountain ranges which extends entirely across the continent, and which would enable the traveler to cross from the Atlantic to the Pacific, without necessarily surmounting any elevation greater than four thousand feet.[1] The southeastern range is the highest, and the culminating point is said to be the Cerro de Cuiteco, sixty leagues northeast of Jesus Maria, on the western border of Chihuahua. The approximate altitude of the Cumbre de Basascachic is 7429 feet, and that of Guadalupe y Calvo, 7825 feet.

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[1] See Emory, in Mexican Boundary Report, vol. i, p. 41.

To the north, the ranges east of Sahuaripa are also very high; but they have never been measured. No peaks or ridges, however, in this portion of Mexico attain anything like the elevation of the higher portion of the Sierra Nevada, few if any points exceeding 10,000 feet in altitude.

"The direction of the Sierra is nearly that of a line connecting some of the best mining districts in Mexico, which are situated on or very near the summit of the mountains. These districts are the following, enumerating them in their geographical order from the south toward the north: In Durango, San Antonio de las Ventanas, Guarisamey, and San Dimas, remarkable for their auriferous silver ores, and sixty-two Mexican leagues northeast of Mazatlan; in Chihuahua, Guadalupe y Calvo, and San Pedro de Batopilas, yielding fine specimens of native silver; also, Jesus Marie, in the same State, and the Real del la Cieneguita, Sonora, with silver and gold mines.

"The geological structure of the occidental slope of the Sierra Madre, as well as that of the other parts of this great chain, is exceedingly interesting, and, as yet but little known, notwithstanding the valuable investigations of Humboldt and other eminent men; for, up to the present time, the age of the different formations has never been fixed with any degree of accuracy, from want of materials and of sufficient observations. In 1863, 1864, and 1865, however, I explored quite a number of localities in northwestern Mexico, and was thus enabled to obtain a pretty good general idea of the geology of that region; and, in Sonora, to which my attention was especially devoted, I succeeded in finding fossils in sufficient quantity to allow of the determination of the age of the principal formations of the northern Sierra Madre. By tracing the connection of these rocks with those of Central Mexico, additional light will be thrown on those districts of which, at present, but little is definitely known.

"The igneous rocks, which occur more abundantly on the Pacific slope, are granites, either fine or very coarse-grained; porphyries, more or less feldspathic; and greenstones, all of which are cut by numerous dikes of extremely varied character. The granites, however, are very poor in veins of the precious metals, while the porphyries are highly metalliferous. In Sinaloa (Candelero) and Durango (San Dimas) we see that the granites underlie the metalliferous porphyries, and that the greenstones, in Sonora (near Hermosillo and in the vicinity of La Haciendita), penetrate through them.

"The oldest sedimentary rocks, which I have observed, belong to the Carboniferous series; this is represented in the eastern part of Sonora, by heavy masses of limestone, forming very high and rugged ridges, running a little west of north. The upturned strata are seen, in many places, to rest on granite. Argentiferous veins occur throughout this formation.

"The next group of sedimentary rocks, in order, is the Triassic; this forms isolated mountain groups in Sonora, and offers an interesting field for investigation. Instead of limestones, it is made up of heavy beds of quartzites and conglomerates, with coal-bearing clay shales; all of these are disturbed and elevated, and rest on greenstones, feldspathic porphyries, or granite. Wherever metamorphosed, the Triassic rocks are auriferous and contain veins of silver ores. The metamorphic slates and limestones of the Altar and Magdalena districts, which include the richest gold placers of Sonora, may possibly be of Triassic age; but the fossils collected are too imperfect to admit of this being determined. There are some reasons for believing those rocks to be rather of Jurassic than of Triassic age, as they differ in lithological characters from both the Triassic and Carboniferous of northern Mexico, resembling, rather, the Jurassic gold-bearing slates of the Sierra Nevada, in California; besides, they lie outside and to the west of the Sierra Madre. It may also be noticed that the gold which they furnish does not resemble that obtained from the Triassic strata.

"The Cretaceous period is also represented at the foot of the Sierra Madre, at Arivechi, in Sonora. The strata belonging to this series are chiefly argillaceous shales, and they rest upon porphyries and Carboniferous limestone. They have been disturbed and elevated since their deposition. The fossils, which they contain in great number and in a fine state of preservation, will be noticed farther on.[1]

"All the above-mentioned formations were already in existence before the first eruption of the volcanic rocks took place. These latter are found scattered along the whole Pacific coast, and extend from the Gulf of California up to the very summit of the Sierra. It is very interesting to see the volcanic formations spread over so extensive a region, especially as there are no active volcanos known in northern Mexico, and not even any indications of ancient craters or vents.

"The lithological character of the eruptive materials is extremely varied, and there seem to have been several periods of igneous action preceded by as many disturbances of the strata, all of which took place after the close of the Cretaceous epoch. Three different series of volcanic rocks may be observed in Sinaloa and Sonora, unconformable with each other: and these again may be subdivided into groups, after a thorough examination has been made of the extensive suite of specimens which has been collected. The lower or oldest series affords several hundred varieties of porphyries, characterized by crystals of feldspar or augite. There are also very peculiar trachytic rocks, resembling granite in appearance. These volcanic materials occur in beds or in masses, and are frequently cut by dikes; but they are quite destitute of veins containing gold or silver, the only metalliferous ores they contain being those of copper (?) and iron, and these in small quantity. Various volcanic ridges in Sonora belong to this class. The second series consists of extensive beds of micaceous, trachytic tufas, and breccias, all more or less uplifted since their deposition, and covering the different igneous and sedimentary formations as well as the older volcanic porphyries. These attain a great thickness, between San Dimas and San Ignacio, in Durango and Sinaloa.

"Above these formations occur ancient alluvial deposits, with bones of extinct animals (elephants) at two localities: near La Noria, northeast of Mazatlan, and in the Arroya de la Palma, two leagues east of La Casita, in Sonora.

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[1] "Several species have been identified by Mr. Gabb as already described from Texas, and figured by Roemer in "Die Kreidebildungen von Texas;" these are, *Ammonites pedernalis* von Buch, *Natica pedernalis* Roem., *Turritella seriatim-granulata* Roem., *Gryphæa navis* Hall, *Cyphosoma Texanum* Roem., *Eulima Texana* Roem. Besides these, two other species are identified, viz.: *Cardium multistriatum* Shum., and *Turbinolia Texana* Con."—*Report*, p. 11.

"Sheets of basaltic lavas, somewhat similar to those of California, and probably of the same age, forming with tufas the upper volcanic series, overlie the other formations, occupying a nearly horizontal position.

"The most recent formation is that of the terrace deposits of sand and gravel, which occur in Sonora."

The pamphlet, after giving details on these several formations, closes with a list of the principal mines of northern Mexico, in which the ores they yield are mentioned, the dip and strike of the veins, and other particulars of interest.

2. *On Fucoids in the Coal Formation;* by LEO LESQUEREUX. 14 pp. 4to, with a plate. From the Trans. Am. Phil. Soc., xiii, 313.—After remarks on the very great rarity of Fucoids, or remains of any *marine plants*, in the Coal formation, Prof. Lesquereux describes a species found on Slippery Rock Creek, opposite Wurtemberg, Lawrence Co., Pennsylvania. The specimens were from the lower surface of a thin layer of limestone, immediately above a bed of coal 6 to 18 inches thick. The limestone, 1 to 1½ ft. thick, is overlaid by shales and sandstone 20 ft.; above this, fire-clay 2 ft.; limestone 3 ft.; shales 150 ft., containing some thin beds of stigmaria fire-clay, and in some parts branching cylindrical Fucoids, resembling *Palæophycus tubularis* Hall; micaceous sandstone, with a few remains of Palæophycus.

The frond, in the new species, is somewhat lyre-shape in outline, but varies much. It is 2 in. to 1 ft. in length, and half this in breadth. It has a fleshy margin from an eighth to a fourth of an inch thick; and it is crossed by curving ribs, which pass from the inner to the outer edge, nearly concentric with the lower margin.

This species is referred by Lesquereux to Sternberg's genus *Caulerpites*, and named *C. marginatus*. It resembles somewhat *Fucoides Cauda-Galli* of the Devonian,—Hall's *Spirophyton*—which Lesquereux refers to the same genus. It has nothing of the spiral character of the *F. Cauda-Galli*, on which Hall bases his name *Spirophyton*. Lesquereux considers this character not of generic value, and due only to a twisting of the frond as it grows—a peculiarity observed in some living Fuci.

Lesquereux closes his memoir with a statement of some strong reasons for believing that petroleum has been derived mainly from the decomposition of *marine* plants.

3. *On the oldest known British Crab (Protocarcinus longipes* Bell, MS.) *from the Forest Marble of Malmesbury, Wilts;* by HENRY WOODWARD.—The author stated that three genera and twenty-five species of Brachyurous Crustacea had already been described by Prof. Reuss and H. von Meyer from the Upper White Jura of Germany; but as no limbs or abdominal segments had been met with, it was more doubtful where to place them than the species now described, which had nearly all its limbs *in situ*, and a portion of the abdomen united to it. *Protocarcinus* closely resembles the common spider-crabs—the *Maidæ* and *Leptopodidæ* living on our own coasts.—*Proc. Geol. Soc., Reader*, June 2.

4. *Memoirs of the Geological Survey of Great Britain and of the Museum of Practical Geology. The Geology of North Wales*, by A. C. Ramsay, F.R.S., Local Director of the Geological Survey of Great Britain. 382 pp. 8vo, with numerous plates, a map and sections. London, 1866. 13*s*. in boards.—In a brief introductory notice of this volume preceding the Preface, Sir Roderick Murchison says, "The Memoir upon the Geological Structure of North Wales which is now published is, I consider, the most important work which has been issued by the Geological Survey during the ten years that have elapsed since I became Director;" and we would add our testimony to the great value of the work. It treats of the earliest fossiliferous rocks of Wales, and with great fulness and exactness of description. There are 26 lithographic plates of fossils, besides sections, and a beautiful colored geological map of Wales. In the summary at page 229, Prof. Ramsay gives the following statement respecting the lowest of the Silurian beds.

"The chief object of this Memoir has now been accomplished, for I have described in detail the Cambrian and Lower Silurian rocks of Merionethshire, Caernarvonshire, and Anglesey, and in a briefer manner the Upper Silurian and other strata that lie strictly within the region I proposed to illustrate. I shall now, in conclusion, revert to some leading stratigraphical and paleontological points, by way of summary, and also partly to show the general connexion of the region described with other parts of Wales, in such a way as to explain the effect of the whole on its physical geography.

"First, then, the term Cambrian has been applied by the Geological Survey only to those strata that lie directly below the Lingula flags, and which, excepting worm-burrows, have heretofore yielded only doubtful fossils. These are the oldest strata in Wales, and are believed to be the equivalents of the Irish rocks at Bray, and of the red conglomerates and sandstones in the northwest of Scotland described by Sir Roderick Murchison. In Wales, however, we never get to their base, and whether or not they lie unconformably on gneiss, like that of the Lewes and the St. Lawrence, it is vain to speculate.

"The relation, however, of the Cambrian to the overlying strata is clear, for everywhere in Wales there seems to be conformity, and even a gradual passage from the Cambrian rocks to the Lingula flags. They are, therefore, intimately related to each other, and perhaps, except for the convenience of a great lithological distinction, they scarcely require separation by line and color.

"The Lingula flags, from 5,000 to 6,000 feet thick where thickest, contain, as at present known, about 22 species of Trilobites of the genera *Dikelocephalus* (4), *Agnostus* (5), *Olenus* (7), *Conocoryphe* (*Conocephalus*) (3), *Ellipsocephalus* (1), and *Paradoxides* (2); *Hymenocaris vermicauda*, and 3 Brachiopoda (2 *Lingulæ* and an *Orthis*), 1 Polyzoon (*Dictyonema*), and several Annelids.

"Above the true Lingula flags lie the Tremadoc slates; and Mr. Salter first proved that the fossils of these beds are mainly distinct from those of the Lingula flags below and of the Llandeilo and Bala beds above them. Thus of 11 genera of Trilobites only Dikelocephalus, Conocoryphe, Olenus, and Agnostus, are common to the Lingula flags, and the species are entirely distinct. The remaining seven are *Angelina*, *Asaphus*, *Cheirurus*, *Ogygia*, *Ampyx*, *Psilocephalus*, and *Niobe*. The Pteropod *Theca*, and *Bellerophon*, *Conularia*, *Orthoceras*, and *Cyrtoceras*, as far as we know, first appear in the Tremadoc slates in Britain. Of the Trilobites, *Agnostus princeps* seems to be the only species common to Lingula flags and Tremadoc slate, and of a tolerably numerous list of bivalve shells *Lingulella Davisii* and *L. lepis* are the only forms that ascend from the lower horizon. It was not till after the whole of Wales had been mapped that the existence of the Tremadoc slate as a recognizable subformation was suspected, for where almost all the rocks are slaty, and where there is no visible break in conformity, minor lithological distinctions are generally of small value. All known evidence, however, tends to prove that in Wales the Tremadoc slate is a very local formation, and though searched for, none of its peculiar fossils have yet been found in Wales, except in certain spots in Merionethshire and Caernarvonshire.

"Next come the Llandeilo and Bala beds, the prodigious development of life in which had no parallel in the older British formations; and it is important to remember that the fossils of these strata are to a great extent different generically, and almost entirely specifically, from those known in the more ancient formations.

"With respect, then, to Lingula, Tremadoc, and Llandeilo and Bala beds, taking into consideration the remarkable breaks in succession not only of species but of genera, together with various physical points of great significance, I have no doubt that actual unconformity exists in this part of the series, and that there is a necessary connexion between these facts. Indeed, this unconformity, if not seen, is, as already stated, easily inferred, for while in Merionethshire the Lingula flags are from 5,000 to 6,000 feet thick, only 11 miles north, near Llanberris, their thickness is only 2,000 feet, this reduction having been produced probably by *unconformable overlap*. Close to Menai Straits, if present at all, the Lingula beds are still thinner, and in Anglesey they are absent altogether, so that the Llandeilo and Bala beds lie directly and, I believe, unconformably on Cambrian strata. To show that this is not a mere local accident, let me recall the circumstance that in Ireland and in Sutherlandshire the Lingula flags are also absent, and Llandeilo beds lie unconformably on Cambrian grits and conglomerates."

Professor Ramsay continues with a summary of his results with regard to the rest of the Silurian.

The volume closes with an appendix on the fossils (to which the plates of fossils pertain) by the able paleontologist, J. W. Salter.

5. *Notes on the formation of the Dead Sea;* by L. LARTET.—The memoir on the Dead Sea by Mr. Lartet closes with the following conclusions:

In reviewing my geological study of the basin of the Dead Sea I am led to think—

(1.) That at the end of the Eocene period, and in consequence of an upward movement (the date of the commencement of which cannot be determined) an ocean bed was protruded corresponding to the continent of Syria and Arabia Petrea.

(2.) Before this protrusion (even before the deposit of the Cretaceous rocks), disturbances had taken place in the submarine beds, and a fissure had opened from north to south through which the feldspathic porphyries made their way, which now appear between Petra and the Dead Sea.

(3.) This fissure may have been prolonged toward the north by subsequent movements which determined the formation of the highlands of Palestine; while the fall of the eastern side of those highlands all along the line of dislocation, may have caused that narrow and lengthened depression which separates Palestine from Arabia.

(4.) The basin of the Dead Sea has thus been formed without any influence from or communication with the ocean; whence it follows that the Lake which occupies the bottom of the basin has never been anything but a reservoir for the rainfall—the saltness of which originally proceeded from the constitution of the environs of the Lake, and has greatly increased under the influence of incessant evaporation.

(5.) Toward the end of the Tertiary period, or the commencement of the Quaternary period, the water of the lake stood at more than 100 meters above its present level, and then deposited marls rich in salt and gypsum beds.

(6.) At a later period volcanic eruptions have taken place to the northeast of the basin, which produced important streams of basalt, some of which extend as far as the Jordan valley itself. Other eruptions of less importance took place directly east of the lake, of which three reached its eastern shore near the Wadys Ghuweir and Zerka Main and the south end of the plain of Zarah.

(7.) Hot and mineral springs, bituminous eruptions, similar to those which accompany and follow volcanic action, and earthquakes, which are still frequent in the district, have been the last important phenomena affecting the basin of the Dead Sea.—*Reader*, *April* 14.

6. *On the occurrence and geological position of Oil-bearing deposits in New South Wales;* by the Rev. W. B. CLARKE.—The author first described the oil-producing schists and cannels of New South Wales as they exist at Colley creek, at the head of the Cordeaux river (Illawarra shales), at various places in the Wollondilly and Nattai valleys, at Reedy creek (Hartley cannel), Stoney creek, and elsewhere; as well as a substance resembling "Bog-butter," occurring at Bournda, and probably of very recent date. Respecting the Colley creek cannel, Mr. Clarke observed that he saw no porphyry near it, but that a seam or mass of the cannel, which here contains numerous scarcely rounded grains of quartz, was passed through in the midst of a series of layers of black, partly unctuous clay, which also contained many similar quartz grains; these grains gave to the clay a porphyritic aspect, so that by sight alone one might be led to consider them a decomposed porphyry. The chief conclusions at which the author arrived were, (1) that, with the exception of the Stoney creek cannel, all the oil-producing deposits occur in the Upper Coal-measures, and that the cannel of Stoney creek, on the river Hunter, occurs in the Lower Coal-measures, which are above the Lower Marine beds with Trilobites, below which again are numerous fossiliferous beds before the porphyry is reached; and (2) that the cannel belongs to beds in which *Glossopteris* occurs, and therefore may be a slight additional evidence to their antiquity, as it is an analogue of the "Bog Head" cannel of Scotland.—*Proc. Geol. Soc.*, *Reader*, *Apr.* 21.

7. ***Report on Geological and Industrial Resources of the Grand Traverse Region, or the Counties of Antrim, Grand Traverse, Benzie and Leelanaw, in the Lower Peninsula of Michigan;*** by ALEXANDER WINCHELL, A.M., Professor of Geology, Zoology and Botany in the University of Michigan, and late State Geologist. 98 pp. 8vo, with a map. Ann Arbor, 1866.—The object of this pamphlet by Professor Winchell is to present to view the resources of the Grand Traverse Region—that is, the portion of Michigan on the west side of the peninsula about Grand Traverse Bay. It treats of the topography, soils, climate, timber and native useful plants and animals, geology and geological resources as regards salt, petroleum and clays, and of the farm crops, fossils, etc. Professor Winchell closes his Report with an Appendix, giving some detail with regard to the Hamilton rocks of the region, and descriptions of a considerable number of new fossil corals and mollusks. He divides the formation into (1) Pale buff massive limestones (consisting of *Pleurotomaria* beds below, as he designates them, and *Stromatopora* beds above; (2) Bituminous shales and limestones (consisting of *Bryozoa* beds below and of *Acervularia* beds above); (3) Buff vesicular magnesian limestones; (4) Chert beds. No. 1, or the lowest, graduate, on the east side of the peninsula of Michigan, into the subjacent Corniferous limestone. The total number of species of fossils observed in No. 1 is 41; in 2, 87; in 3, 8 species.

8. *New mineral localities;* by GEO. J. BRUSH.—(1.) *Diaspore.*—A year or more since Mr. W. W. Jefferis sent me some minute fragments of a hard foliated mineral found by him at Newlin, Chester Co., Pa. The substance was imbedded in emerylite, and on examination proved to be diaspore. Quite recently Mr. Jefferis has continued his explorations and has discovered the diaspore in crystals, some of which he informs me are 1 to 1⅞ inches in length, ⅜ to 1⅛ broad, with a thickness of an eighth to a sixteenth of an inch. The only crystal I have seen is nearly honey-yellow in color, and has a high luster, especially on the plane $i\breve{\imath}$, or that parallel to the cleavage; it is imbedded in emerylite so that it is difficult to make out the other planes. In size the crystals surpass any that I have seen from Asia Minor or Schemnitz, and in perfection of planes they compare favorably with the beautiful crystals discovered by Prof. J. Lawrence Smith at Gumuch Dagh.

(2.) *Ouvarovite.*—Among some specimens presented to the metallurgical collection of Yale College by Mr. Clayton of San Francisco, there is a piece of chromic iron, from near New Idria, California. This specimen is covered with rhodochrome, and with a green mineral which was supposed by the collector to be emerald nickel. On examination with a magnifier the latter proves to be in druses of crystals showing the rhombic faces of the regular dodecahedron; and on blowpipe analysis the mineral was found to have the pyrognostic characters of chrome-garnet. Its association is precisely the same as that of the chrome-garnet of the Urals.

9. *On crystallized Cryolite;* by G. HAGEMANN. (Communicated for this Journal.)—Crystals of cryolite have hitherto been considered a great rarity, and the only form described is that of the simple rectangular prism. They have been much sought for with little success, and great was my surprise in finding them abundant in the cryolite of a recent cargo.

The cryolite on which the crystals were found is not of good quality, that is to say, it is much mixed with other minerals. The purer cryolite cargoes have never afforded me any traces of crystals. They occurred over the exterior of the large masses, and only in one instance have I found them lining a cavity. They were covered in all cases with a red mineral.

The most common form is a rectangular prism, either short and tabular, or long; the former sometimes 6 mm. square; the latter small, not exceeding 3 mm. in length. The prisms have occasionally a replacement of two of the angles by a triangular plane (*a*); and the base and sides are diagonally striated in a direction parallel to the sides of this plane, and in the same direction when the plane is absent. The prisms are grouped one over another, giving a stair-like surface, which is mostly covered by the red mineral alluded to. Where this mineral is absent, or has been removed, the crystals are perfectly clear and colorless. In other instances the prismatic crystals shoot up through a crust, about 1 mm. thick, covering the pure cryolite. This crust has an opal-like aspect, and is colorless, and on breaking it over the clear cryolite crystals, it can be seen that there is a passage from the underlying cryolite to the surface through this opal-like mineral.

The greatest variety in the crystalline forms were found among the crystals lining the cavity, some of which are 4 mm. through, though these are mostly broken and incomplete. The best are rectangular prisms with triangular planes on all four angles of the base, corresponding to two domes (*a*, before described, and *b*); but which of the two is the macrodome has not yet been made out. By measurement with the common goniometer the prismatic angles were found to be 90°, or at least very near this. The angle of dome *a* over the summit was found to be 70° 30′; the planes of dome *b* were too small for my measurement. On another crystal I obtained for a prism 128°; and for a dome 108° 30′; the crystal was a very small one and the measurement therefore imperfect; the calculated angles are respectively 126° 50′ and 109° 16′. Planes of other domes and pyramids exist, but they are all very small. However incomplete this examination, there can be no doubt that the crystals are trimetric in character.

10. *Paracolumbite and Corundophilite of* C. U. Shepard.—We have received from Prof. Shepard a communication in which he states that the *paracolumbite* analyzed by Pisani (see this Jour., vol. xxxvii, 1864) was not from Taunton, the original locality, but from Cumberland, Rhode Island, where a mineral occurs so closely resembling it that he himself had later pronounced it paracolumbite. Pisani's specimen, thus labeled, was from Prof. Shepard. Prof. S. claims that *true* paracolumbite has not yet been analyzed.

Prof. Shepard makes a similar claim, in this communication, with regard to *his* "*corundophilite*" of Chester, Mass., which has been analyzed by Pisani and other chemists and shown to be *clinochlore*. He says that the *true* corundophilite is that of Asheville, N. C., of which he had only 0·146 of a gram for a chemical examination.

But Prof. Shepard has published in this Journal and elsewhere that the Chester mineral is corundophilite; he has written Dr. J. Lawrence Smith to this effect (as cited in our last number), and other persons also, including one of the editors of this Journal; and he has distributed specimens so labeled. And if he does not know his own species, it may well be inferred that it is because they have no distinctive characters. The species was founded by him on an imperfect examination with an analysis in which there was a loss of 20 per cent, and never should have been named without more knowledge.—J. D. D.

11. *Color of a diamond changed by heat.*—In May last, Prof. Fremy exhibited to the Academy of Sciences at Paris a yellowish diamond, of the size and quality that ordinarily sells for 12,000 dollars, which on being heated changes its color to rose-red; this color it retains for two or three days and then gradually resumes the original yellow. On account of this peculiarity the actual value of the diamond was stated to be three times the amount above mentioned.—*Les Mondes*, p. 85, May 10, 1866.

12. *Gieseckite a result of the alteration of Elæolite.*—The view that the mineral gieseckite, found first in Greenland and some years since at Diana in New York, is a result of the alteration of elæolite or nepheline has been held by different authors. Pisani, of Paris, has recently observed that the elæolite of Brevig, Norway, is often partly altered to a brick-red material which is true gieseckite in nature and composition. On the same specimen are found true translucent elæolite, affording only 0·33 p. c. of water on calcination, and entirely soluble in acids; alongside of this, there are red spots where alteration has commenced; and beyond, the mineral is changed to a brick-red uniform material, mostly opaque, with some translucent spots of unaltered elæolite. This red material afforded 5·90 p. c. of water, and dissolved only partially in dilute nitric acid, it yielding an abundant red precipitate. On separating the insoluble portion, by treatment with dilute cold nitric acid, this afforded, on analysis, $\dddot{\mathrm{Si}}$ 46·95, $\dddot{\bar{\mathrm{Al}}}$ 34·65, $\dddot{\bar{\mathrm{Fe}}}$ 1·86, $\dot{\mathrm{Mg}}$ 0·58, $\dot{\mathrm{Ca}}$ 0·68, $\dot{\mathrm{K}}$ 8·71, $\dot{\mathrm{Na}}\dddot{\mathrm{Si}}$ 0·71, $\bar{\mathrm{H}}$ 5·58=99·72, thus showing that, besides taking up water, the soda of the elæolite had been replaced almost wholly by potash. This transformation of elæolite into gieseckite is similar in many respects to that of cancrinite into bergmannite elucidated by Pisani and Sæmann in 1862 (Ann. de Ch. et Phys., lxvii). The facts prove that these apparently crystallized minerals are actually pseudomorphs. Blum has observed a specimen of bergmannite with a nucleus of elæolite, showing that bergmannite may come from the alteration of this mineral as well as cancrinite. Yet as the crystals of bergmannite are usually quite long, unlike those of nepheline, it is probable from the occurring forms that it is mostly derived from cancrinite.—*Les Mondes*, June 28, 1866, p. 368.

13. *Apophyllite made by artificial means.*—Becquerel has observed that if distilled water is made to run slowly over plates of sulphate of lime, the surface becomes chatoyant from the dissolving action of the water; and if a saturated solution of sulphate of potash be employed instead of water, a double sulphate of potash and lime is obtained, crystallized in needles; while with a solution of silicate of potash (marking 0 to 10 areometric degrees), instead of the sulphate, pearly radiated crystals are formed which are a double silicate of potash and lime, and have all the characters of apophyllite.—*Les Mondes*, July 5, p. 415.

14. *On a new variety of Spinel;* by H. St.Claire Deville.—A black spinel with low pyramids in place of the octahedral planes, and the faces rounded, has been found in the rock of Auvergne called *Lherzolite.* Most of the crystals, however, are octahedrons with rounded beveled edges and eroded faces. The crystals are 5 to 10 (rarely 20) millimeters in diameter; and though mostly black are sometimes reddish-brown. G. = 3·871 for the black; 3·868 for the reddish-brown. Analysis by Deville gave $\bar{\mathrm{Al}}$ 59·06, $\bar{\mathrm{Fe}}$ 10·72, $\dot{\mathrm{Fe}}$ 13·60, $\dot{\mathrm{Mg}}$ 17·20 = 100·58, whence the formula $(\dot{\mathrm{Mg}}, \dot{\mathrm{Fe}})(\bar{\mathrm{Al}}, \bar{\mathrm{Fe}})$, as in true pleonaste.—*Les Mondes*, July 12, p. 458.

15. *Origin of the Diamond.*—E. B. de Chancourtois has presented the view that the diamond has been formed from hydrocarburetted emanations, as sulphur is formed from hydrosulphuretted emanations, and that its origin is thus connected with the previous existence of petroleum-bearing or bituminous schists. In the oxydation of sulphuretted hydrogen in solfataras, all the hydrogen is oxydized, but only part of the sulphur passes to the state of sulphurous acid in this humid process of combustion. So, in an analogous manner, the diamond was probably formed; that is, in the course of the humid combustion of a carburetted hydrogen in which all the hydrogen was oxydized, but only a part of the carbon was transformed into carbonic acid. This view accords with the occurrence of the diamond in arenaceous rocks or itacolumites, which are mostly metamorphic rocks of paleozoic age, and which may have once been bituminous either by original formation or by emanations from lower rocks. Mr. Chancourtois supposes that the crystal would have formed only where there were fissures for the passage of the vapors of the carburetted hydrogen, and where the process could go on with extreme slowness.—*Les Mondes*, July 19, p. 438.

[The author does not appear to connect the process of formation with that of the metamorphism to which the diamond-bearing rocks have undoubtedly been subjected, and which may have been essential to the result.]

16. *Paragenesis of Minerals.*—Reuss has a paper of great interest in the Berichte of the Vienna Academy for Jan. 7, 1863, on the associations and superpositions of the various minerals at Przibram, with reference to their origination at different periods and in various successions. The great number of metallic ores and other species in that noted mining region makes it especially instructive in this respect.

17. *Cassiterite.*—Cassiterite from Montebras in France has been found by Capt. Carron to contain two to three per cent of niobic and tantalic acids, and in some cases even five per cent, and he says that the ore may be used for obtaining these rare metals.

Cassiterite is found in the Temeschal Ranche, in Los Angeles Co., California, at several points, and with some promise of economical value. It is associated in some cases with a ferruginous porphyritic rock, or a very black compact hornblende in granite and quartzite.

Wood-tin has been found also near Boonville, Owhyhee Co., Idaho Territory, by Walter Gibson of New York.

18. *Analyses of Minerals;* (communicated by S. B. Sparkler.—(1.) *Hornblende from Birmingham, Delaware Co., Penn.* This rock is crystalline in structure, and forms a bed one to two miles long and from half

to three-fourths of a mile wide. The analysis gave: Silica 47·77, FeO 15·41, MnO ·26, Al_2O_3 7·69, CaO 13·16, MgO 15·28=99·57. It is of a very dark green color.

(2.) *Precious serpentine from East Goshen, Chester Co., Penn.* Silica 43·89, FeO 1·38, MgO 40·48, HO 13·45=99·20.

19. *On Anatase at Smithfield, R. I.;* by Rev. E. B. EDDY.—Anatase occurs at the Dexter Lime Rock, Smithfield, R. I., and is there associated with crystallized quartz, nacrite, acicular natrolite and pearl spar. The rock is dolomite. Needles of natrolite penetrate the quartz crystals in every direction, and the calcite also.—*Bost. Soc. Nat. Hist.*, x, 94.

20. *Petroleum in Russia.*—A well, six centimeters in bore, near Temrioux, in southern Russia, affords 73,000 litres of oil per day.

21. *On the Composition of the Stone implements found in Celtic monuments.*—Damour has examined the stone relics of various museums and private collections of France and Switzerland, and finds that the following minerals have been used for them: quartz (agate, jasper, flint); obsidian, fibrolite, oriental jade, or nephrite, Oceanian jade, jadeite, a rock which he calls chloromelanite, hornblende (either actinolite or black hornblende), saussurite, besides the rocks aphanite, basalt, diorite, dolerite, petrosilex. The obsidian comes from volcanoes, and is found in Europe at Cantal in France, in Hungary, near Naples, and at Milo and Santorin in the Grecian Archipelago. Fibrolite (Sillimanite) occurs in Europe in the Tyrol, in Moravia, Bavaria, and also in France in the departments of the Rhone, and the Haute-Loire. Damour concludes that some one or more of the French localities have afforded the fibrolite.—*Les Mondes*, Nov. 2, 1865.

The variety of fibrolite which seems to have been preferred by the Celts for their stone implements has been supposed, according to B. de Lom, to occur only in the vicinity of Issoire, where it is found in boulders. This author adds other localities of the variety, in the canton of Paulhaguet, and in the vicinity of Chavagnac and Ourouze, associated with mica, kyanite and red or blue corundum, and also between St. Eble and Crespignac. He mentions masses of fibrolite weighing eight to twelve kilograms. With it occurs andalusite, a mineral hitherto not found in France.—*Les Mondes*, Dec. 21, 1865.

22. *Geological Survey of Iowa.*—It gives us pleasure to state that the geological survey of Iowa has been again taken up by the State, and that $13,000 have been appropriated for two years, and Dr. C. A. White put in charge. Dr. White will we believe do well the work before him and produce Reports of great value both to the state and to science. The terms of his appointment impose on him and his assistants the duty of giving "the people of the State the greatest amount of practical information in relation to its resources;" and require the state geologist to publish popular articles in the newspapers of the state; also to deposit in the State University all type specimens, and in the Agricultural College all those illustrating economic geology.

23. *New Paleozoic Crustacea and Cirriped.*—Mr. HENRY WOODWARD has described in the Journal of the Geological Society of London for November, 1865, two new species of Devonian Crustaceans of the Eurypterus family, and genus Stylonurus, and a third, between Limulus and

Eurypterus in character, obtained from the Lower Ludlow beds, for which he proposes the generic name *Hemiaspis*. He also shows that the *Chiton Wrightianus* of de Koninck, from the Wenlock beds, is not a Chiton but a Cirriped, and he names the genus *Turrilepas*. This is the only species of Paleozoic Cirriped yet discovered, and has therefore great geological interest. The remains of these species are figured on two plates.

24. *The Geological Magazine, or Monthly Journal of Geology* (with which is incorporated "The Geologist"); edited by HENRY WOODWARD, F.G.S., F.Z.S., Professor JOHN MORRIS, F.G.S., &c., and ROBERT ETHERIDGE, F.R.S.E., F.G.S., etc.—The third volume of this Magazine of Geology, published by Messrs. Trübner & Co., London, commenced with January of the current year. It is issued in monthly numbers of 48 pages each, illustrated by plates and woodcuts. It has an able editorial corps, and numbers many of the first geologists of England among its contributors; and the interest and value of its papers entitle it to a large American circulation. A plate in No. 3 (March) gives excellent views of the wings of a Libellula from the Stonesfield beds, illustrating a paper by Prof. John Phillips; two others, representations of the jaws and teeth of a new Sauroid fish from the Kimmeridge Clay, described in an article by Prof. Owen; and another, sections of an ancient beach and submerged forest near Calais. In No. 5, Prof. Owen has an illustrated paper on a small mammal from the Upper Oolite of Purbeck which he calls *Stylodon pusillus*, the specimen of which is part of the lower jaw with the teeth, yet is insufficient for a decision as to whether it is Marsupial or not. Besides original papers, this excellent magazine gives reports of the geological papers in the Proceedings of different societies, and reviews of new works. The price per number is 1*s.* 6*d.*

III. BOTANY AND ZOOLOGY.

1. WILLIAM HENRY HARVEY, whose lamented death was announced in the last number of this Journal (p. 129), was born at Summerville, near Limerick, Ireland, on the 5th of February, 1811. His father, Joseph M. Harvey, was a highly respected merchant in that city, and a member of the Society of Friends. William Henry was, we believe, the youngest of several children. He received a good education at Ballitore School,—an institution of the Friends, and on leaving it was engaged for a time in his father's counting-room, devoting, however, all his spare time to Natural History, his favorite pursuit even from boyhood. He made considerable attainments in Entomology and Conchology, and in Botany he early turned his attention to Mosses and *Algæ*. To the study of the latter, in which he became preëminent, he was attracted from the first by the opportunities which he enjoyed on the productive western coast of Ireland, the family usually spending a good part of the summer at the sea-side, mostly on the bold and picturesque shore of Clare. As the late Sir Wm. Hooker's bent for botany was fixed by his accidental discovery of a rare moss, which he took to Sir J. E. Smith, so in turn was Harvey's, by his discovery of two new habitats of another rare moss, the *Hookeria lætevirens*, which led to a correspondence with Hooker, and to a life-long mutual attachment of these most excellent men. Encouraged

by his illustrious friend and patron, Harvey sought some position in which he might devote himself to science; and it would appear was selected by Mr. Spring Rice (the late Lord Monteagle), for the post of Colonial Treasurer at the Cape of Good Hope; that by some accident the appointment was made out in the name of an elder brother, and an inopportune change of Ministry, frustrated all attempts at rectification. There was no other way but for the brother to accept the undesigned appointment, and take the young botanist with him to the Cape as his assistant. This was done, and the brothers sailed for that colony in the year 1835. But the health of the elder brother suddenly and hopelessly failed within a year, and he died in 1836 on the passage home. William Harvey's appointment to succeed his brother had been sent to the Cape while he was on his homeward voyage: he immediately returned to his post, and fulfilled its duties for three years, devoting his mornings to collecting and his nights to botanical investigation, with such assiduity that his health also gave way, and he was compelled to return home in 1839. The summer of the next year found him reëstablished, and on his way to the Cape for the third time. But he could not long endure the sultry climate and the intense application; with broken health he came back in 1841 and gave up the appointment.

After two years of prostration and seclusion he was well again; and, in 1844, on the death of Dr. Coulter, he was appointed Keeper of the Herbarium of Trinity College, Dublin. The most important portion of the herbarium then consisted of the collections, yet unassorted, made by Coulter in northwestern Mexico and California. Harvey generously added his own large collections, for which he was allowed fifty pounds a year, in addition to a slender salary, and he proceeded to build up the herbarium into a first-class establishment. The professorship of Botany in the College, which was pretty well endowed, fell vacant about this time; and the College authorities, wishing to elect Harvey to the chair and so to combine the two offices, conferred upon him the necessary degree of M. D. But it was contended that an honorary degree did not meet the requirements, and so Dr. Allman, the present distinguished professor of Natural History at Edinburgh, carried the election.

Except for the slenderness of his salary, Dr. Harvey was now well placed for scientific work, the object to which he wished to devote his life; and he entered upon and pursued his distinguished career henceforth with an entire and well-directed energy that never flagged until he was prostrated by mortal disease.

He had already published, at the Cape in 1838, his *Genera of South African Plants*, hastily prepared, solely for local use, but no unworthy beginning of his work in Phænogamous Botany; and in his favorite department of the science he had brought out, in 1841, his *Manual of British Algæ*, which he re-edited in 1849. He now commenced the first of the series of his greater works, illustrated by his facile pencil,—for he drew admirably. The first (monthly) part of his excellent and beautiful *Phycologia Britannica, a History of British Seaweeds*, containing colored figures of all the species inhabiting the shores of the British Islands, appeared in January, 1846; and the undertaking was completed in 1851, in three (or four) volumes, with 360 plates, all drawn on stone by his own hand. A similar but less extended work, the *Nereis Australis, or Algæ of the Southern Ocean*, which was begun in 1847, was carried only to 50 plates, of selected and beautiful species.

In 1848, Dr. Harvey succeeded Dr. Litton as Professor of Botany in the Royal Dublin Society, to which belonged the Botanic Garden at Glasnevin; this required him to deliver short courses of lectures annually in Dublin or in some other Irish town, and provided a welcome addition to his income.

In 1848, at the request of his friend Van Voorst, the publisher, he wrote his charming little volume, *The Sea-side Book*, the unsurpassed model of that class of popular scientific books; it was published in 1849, and has passed through several editions. In July of that year, having arranged a visit to this country, and having been invited to deliver a course of lectures before the Lowell Institute, he took steamer for Halifax and Boston, passed the summer and autumn in exploring the shores of the Northern States, and in the society of his friends and relatives: for the late Mr. Jacob Harvey, still well and pleasantly remembered in New York, who married the daughter of Dr. Hosack, was his elder brother. In the autumn he gave an admirable course of lectures upon Cryptogamic Botany before the Lowell Institute, in Boston, and afterwards a shorter course at the Smithsonian Institution at Washington. He then travelled in the Southern Atlantic States, continuing the exploration of of our *Algæ* down to Florida and the Keys; and in May, 1850, he returned to Ireland.[1] Under the wise and liberal arrangements made by Prof. Henry, in behalf of the Smithsonian Institution, and with his own large collections augmented by the contributions which every student or lover of *Algæ* was glad to place in such worthy hands, Prof. Harvey now prepared his *Nereis Boreali-Americana, or Contributions to a History of the Marine Algæ of North America.* The work is a systematic account of all the known marine Algæ of North America, but with figures only of the leading species. It was issued in three parts; the first part, the *Melanospermeæ*, in 1852 in the third volume of the Smithsonian Contributions to Knowledge; the second, the *Rhodospermeæ*, in the fifth volume; and the third, or *Chlorospermeæ*, in the tenth volume of the series, published in 1858; and the three parts, collected for separate issue, compose a thick imperial quarto volume, of 550 pages of letter-press and fifty plates. The work remains the principal if not the only guide to the American student of *Algæ*, and one of the most popular as well as useful of the very various contributions to knowledge which the well-managed bequest of Smithson has given to the world.

Before the last part of the *Nereis Boreali-Americana* was published, Prof. Harvey had sought a wider field of scientific labor and observation. Obtaining a long leave of absence, and some assistance from the University in addition to the continuance of his salary, he left England in August, 1853, by the overland route for Australia, stopping at Aden and Ceylon to collect: he visited the east, south and west coasts of Australia, as well as Tasmania. Taking advantage of a missionary ship, which was to cruise among the South Sea Islands, and which offered him unexpected facilities, he visited the Fiji, Navigators' and Friendly Islands, touching also at New Zealand. Returning to Sydney, he sailed to Valparaiso, which he reached much prostrated through over-exertion in a warm climate; and when recuperated he returned home by way of the Isthmus, arriving in October, 1856. The algological collections of these three laborious years, or the Australian portion of them, formed the subject of Prof. Harvey's third great illustrated work, and one of the most exquisite of the kind, the *Phycologia Australica*, the serial publication of which began in 1858 and was concluded in 1863, in five imperial octavo volumes, each of 60 colored plates. All but the last century of plates were put upon stone by the author.

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[1] A notice of Dr. Harvey in the Athenæum states, quite erroneously, that "he also at this time made a tour around the shores of the Pacific, visiting Oregon and California."

Upon Dr. Harvey's return, in 1856, from his long expedition, he found the chair of Botany in the University of Dublin vacated by the appointment of Dr. Allman to that of Natural History in the University of Edinburgh; and he was at once preferred to the position which he had sought when younger and freer, and which he now occupied till his death. The exhausting duties of this chair, and of that which he still held in the Royal Dublin Society, undiminished by the transference to the Government Museum of Irish Industry, did not prevent Prof. Harvey from entering with unabated ardor upon an undertaking of greater magnitude than any preceding one. This was the *Flora Capensis*, a full systematic account of all the plants of the Cape Colony and the adjacent provinces of Caffraria and Natal,—in which he was associated with Dr. Sonder of Hamburgh. Three thick octavo volumes of this work have appeared, the last in 1865, including the *Compositæ*. Along with this Dr. Harvey—learning for the purpose another form of lithographic drawing,—brought out, between the years 1859 and 1864, two volumes of his *Thesaurus Capensis, or Illustrations of the South African Flora*, comprising 200 plates of interesting phænogamous plants. A complete list of his publications would include several contributions to scientific periodicals, mainly to Hooker's Journal of Botany, and a few miscellaneous writings.

In April, 1861, Dr. Harvey married Miss Phelps of Limerick. If not robust, he was apparently in good health, in the full maturity of his powers, and it was hoped only at the noonday of his allotted course of usefulness. But ere the lecture-season of that summer was over, an attack of hæmorrhage from the lungs gave notice of a serious pulmonary disease. Yet he seemed to recover from this almost completely: he resumed his stated work, and gave his lectures as usual in 1863, and also in the spring of the following year, but with some difficulty. The winter and spring of 1864–5 were spent in the south of France, with only transient benefit. Returning to his home and his herbarium he worked on still at the Cape Flora, with cheerful spirit but feeble hands, until he could work no longer. Last spring he sought in Devonshire a milder air, and found a peaceful rest. "On Tuesday, the 15th of May, 1866, at the age of 55 years, he quietly breathed his last, at the residence of Lady Hooker, the widow of his long-attached friend Sir William J. Hooker, surrounded by kind and anxious relatives and friends, and was buried in the cemetery at Torquay on Saturday the 19th of May."

Dr. Harvey was one of the few botanists of our day who excelled both in phænogamic and cryptogamic botany. In Algology, his favorite branch, probably he has left no superior; in systematic botany generally he had now an eminent position. He was a keen observer and a capital describer. He investigated accurately, worked readily and easily with microscope, pencil and pen, wrote perspicuously, and where the subject permitted, with captivating grace; affording, in his lighter productions mere glimpses of the warm and poetical imagination, delicate humor, refined feeling, and sincere goodness which were charmingly revealed in intimate intercourse and correspondence, and which won the admiration and the love of all who knew him well. Handsome in person, gentle and fascinating in manners, genial and warm-hearted but of very retiring disposition, simple in his tastes and unaffectedly devout, it is not surprising that he attracted friends wherever he went, so that his death will be sensibly felt on every continent and in the islands of the sea. A. G.

2. Dr. Robert Kaye Greville, the distinguished predecessor of Dr. Harvey in British Algology, and for many years a prominent investigator and illustrator of other branches of the Lower Cryptogama, the collaborator of Sir Wm. Hooker in the *Icones Filicum*—died at his residence in Edinburgh on the 4th of June last, in the 72d year of his age. His interest in various departments of Natural History was kept up to the last, but of late years he turned his attention mainly to the *Diatomaceæ*. More than twenty years ago, owing to some change in his formerly independent circumstances, he felt it necessary to turn his artistic talents to account, and he took to marine and landscape painting as a profession. Almost throughout his life he was active in various social reforms, he took a prominent part in the agitation against slavery in the British Colonies; and he leaves a highly honored name as a Christian philanthropist as well as Botanist. A. G.

3. *Dr. C. Fournier on Cruciferæ, and Sisymbrium in particular.*—A quarto memoir of 154 pages and two plates, comprising a full monograph of *Sisymbrium* (166 species), prepared by various anatomical researches, and some general views on the arrangement of *Cruciferæ*, forms an unusually imposing thesis, presented to the Faculty of Sciences of Paris, for the doctorate in science, M. Fournier moreover having already that in medicine. In the histological portion (which we merely glance at), the author points out a beautiful radiate striation of the superficial cells of the petals in *Sisymbrium Alliaria* and other species, as also in *Alyssum saxatile;* and the structure of the lining of the cells of the ovary and of the septum is particularly studied. He adopts Lestibudois' view that the septum originates from the tissue of the placentæ, and not from the re-entering borders of the carpels, as DeCandolle supposed.

Touching lightly upon the nature of species, he inclines towards the views of Jordan rather than those of Naudin, believes that the species of a genus are all really circumscribed and definable, and that when two forms differ morphologically even by slight though constant characters, these may often be fortified by equally constant histological differences, the coincidence demonstrating the distinctness of the two types. He develops and makes good use of a principle brought out prominently by M. Duval-Jouve, which he calls "the principle of the parallel variation of congeneric types." It amounts to this quite familiar idea, that the common causes of those ordinary variations which depend upon external influences are dryness on the one hand and moisture on the other. He supposes that almost all varieties are reducible to these two sorts, dryness causing an arrest, and moisture an exuberance of vegetative development, and so the varieties fall mainly under the two types, and take the convenient names, of *xerophilum* and *hygrophilum.* But species also vary more gravely and unaccountably, and in characters which are commonly of specific rank, e. g. in the direction of the pedicels and of the pods, in the length of both and in that of the style, etc. In his exhaustive treatment of a polymorphous European *Sisymbrium,* in which his study of an abundance of forms has led to the reduction of very many nominal species, we gather analogies which, if as favorably placed for the study, our author might probably have advantageously applied to an American group; which we may again refer to.

As to the general arrangement of Cruciferæ, Dr. Fournier moves with decided steps in the direction to which botanists have been tending ever since DeCandolle's re-organization of the family came to be tested. The conclusion is, that the characters drawn from the embryo, at least from the form and position of the cotyledons and radicle, have been overrated; that the primary groups may be advantageously reduced in number (he adds a sketch of a classification down to the tribes which may favorably compare with that of Bentham and Hooker); and that the genera admitted are still too numerous. In the present essay he extends *Sisymbrium* beyond Bentham and Hooker's limits, to comprise *Braya, Halimolobus,* and *Eutrema.* Upon the monograph of *Sisymbrium* we wish to comment upon three or four American species:—

S. Niagarense is founded on a specimen which was collected by Count Castelneau on the banks of Niagara river, and is said to differ from that common introduced weed, *S. officinale* "*fere unico pedunculo filiformi.*" The fruiting pedicels are 2 to 2½ lines long and slender, instead of only 1 to 1½ lines long and thickened. Our local botanists should look after it; but we suppose it is a mere *lusus* of *S. officinale,* which is not indigenous to America.

S. teres. The obscure *Candamine teres* of Michaux was doubtfully referred to *Sisymbrium* in Torrey and Gray's Flora. Dr. Fournier has suppressed the interrogation; but his account of the plant is made up from what was already published, with a considerable error in translating. "Siliques erect, one-third of an inch in length," is rendered "*Siliquæ erectæ, vix tertiam partem lineæ longæ,*" a very short silique indeed, to be, as Michaux states "*breviuscule linearibus.*" As, in the Flora above referred to, the cotyledons are said to be "distinctly incumbent," we are bound to direct attention to a remark in the first edition of Gray's Manual of Botany, p. 34, where it is stated that "the plant appears clearly to be *Nasturtium tanacetifolium* or *N. lyratum* of the Southern States (*cotyledons accumbent!*), which leads me to suspect a mistake in the record of the locality." So far as the portion of an authentic specimen (given to the writer in 1839 by the late Achille Richard) allows the comparison, it accords fully with a plant collected below New Orleans by Dr. Riddell and by Berlandier (his No. 1940). Yet it may be a starveling *N. palustre,* and in that case really gathered at Lake Champlain.

S. Sophia and its relatives. If Dr. Hooker has taken one extreme view in suggesting the union of numerous American forms with this old-world species, Dr. Fournier has certainly gone to the other, and has introduced some new species upon insufficient grounds or wrong conjectures. A review of the materials before us leads to the following remarks upon the North American forms:—

S. canescens Nutt., is our only species with decidedly two-ranked seeds, these being much narrower than the partition. The pods vary from short-oblong or slightly clavate-oblong to oblong-linear and are shorter than their horizontal or sometimes ascending pedicels. *S. brachycarpum* Rich. is a short-fruited form of this, at least in part. *S. Cumingianum* Fisch., is the South American representative, with pods longer than the pedicel; but some of Gillies' specimens might well justify Hooker and Arnott's reference of them to *S. canescens.*

S. incisum Engelm., to which belongs *S. longepedicellatum* (excl. syn.), and probably *S. streptocarpum* of Fournier, is the plant which most approaches *S. Sophia*, having slender pods with one-ranked seeds. We should think it Richardson's *S. brachycarpum* from his phrase "*siliquis linearibus*," and the "*Facies S. Sophiæ, at facile distinguitur siliquis duplo brevioribus*," but the added remarks "4 *lineas longa*, *lineam lata*," and "*pedicello patenti brevior*," indicate *S. canescens.* The pods are only from 4 to 6 or rarely 7 or 8 lines long, and more or less assurgent on widely spreading or horizontal pedicels which are sometimes of their own length or even longer, but are usually considerably shorter. It differs from *S. Sophia*, then, in the shorter and spreading pods. But Mexican specimens, such as Coulter's 683 and Gregg's 408 (referable we suppose to Fournier's *S. Galeottianum*), with their decidedly ascending pedicels and pods, bridge the interval.

S. Hartwegianum Fournier (the Hartwegian specimen of which we unaccountably lack, but we have the Saskatchawan plant of Bourgeau) is known by its short erect-appressed pedicels and pods, the latter short-linear or somewhat fusiform, and 4 to 5 lines long, crowded with seeds in a single rank, or obscurely two-ranked, but each when well developed as wide as the partition, the pedicels a line and a half long. This is Hall's No. 40, from the Rocky Mountains, and was also collected by Burke and by Fremont; doubtless also by Nuttall, (var. brevipes) but we have not his specimen. We should account it distinct, but must admit that a part of Hall's No. 40, and a plant of the same region in Henry Engelmann's collection, effect the transition to *S. incisum.*

S. Sophia L., marked by its elongated linear pods (mostly an inch long), erect or assurgent on ascending pedicels, and one-ranked seeds, belongs chiefly to the Old World, but is naturalized in Lower Canada. The phrase in Torrey and Gray's Flora, "pedicels 4 times the length of the calyx," which shows in Dr. Fournier's opinion that the Quebec specimens cannot belong (as they surely do) to *S. Sophia* (see Bull. Soc. Bot. France, xi, p. 360) was simply copied from DeCandolle's character of that species! We may add that neither dried specimens nor the published figures enable us to see the application of Fournier's character, "*pedicelli erecto-appressi*," or "*jamais son péduncle ne cesse d'être appliqué contre l'axe.*" *S. sophioides* of Fischer, according to Dr. Hooker, is an arctic and seemingly abnormal form of *S. Sophia*, with permanently abbreviated racemes. His father's description of the pods, as 2 inches or more in length, is indirectly contradicted by his figure of the plant "of the natural size," in which the pods scarcely exceed an inch. This is about the length in an arctic specimen, collected by Dr. Seemann, in which the axis of the racemes is considerably lengthened. In it and in some Himalayan specimens of *S. Sophia*, the seeds appear to be oblong instead of short-oval, as they are in the Canadian (introduced) and European plant.

While, therefore, with our present materials, we should acknowledge these four species in North America, we could not affirm that they are strictly circumscribed and definable; and it is quite likely that at Kew we might deem the discrimination hopeless. A. G.

4. *The Genera of Plants;* by RICHARD ANTHONY SALISBURY, F.R.S., etc.—*A Fragment containing part of Liriogamæ.* London: Van Voorst. 1866, pp. 143, 8vo.—The name of Salisbury is familiar, as appended to a good many generic names; but the present generation know little of a botanist who was prominent in the first quarter of the century, who had very much to do in bringing forward the natural system in Britain, but who was too peculiar, too much of an Ishmaelite—hating and being hated—to have justice done him in his own day. Our own impressions of him were derived from the portrait sketched by DeCandolle, in his *Souvenirs.* Dr. J. E. Gray, one of the few surviving naturalists who knew him, has here printed one of his manuscripts, and in a preface has given some account of the man. His name, it appears, was Markham, and he was born in 1761. In 1785 he took the name of Salisbury and ten thousand pounds in the three per-cents from a very old maiden lady of that name, who made him her heir. He died in London in March, 1829. In his turn he proposed to more than one botanist to leave to him his library and his fortune—to DeCandolle, as he tells us for one—on the condition of assuming the name of Salisbury. He actually left a part of his property and his MSS. to the late Dr. Burchell. Since Dr. Burchell's death, two years ago, his sister made over Salisbury's MSS. to Dr. Gray, who intends to present them, when put in order, to the British Museum. He has "here printed one fragment of the '*Genera Plantarum*,' exactly as it was left by the author, for the purpose of showing the kind of work that he intended to produce." In order to secure an exact reproduction of the MSS. the editor read no proof-sheets. He finds, as might have been expected, "that there are a few bitter personal expressions, which it might perhaps have been desirable to expunge." But if the MSS. was to be printed at all, it strikes us that there was no other course to pursue, and no harm can now come of it. *Pleurothallæ* is Salisbury's name for *Monocotyledones*, and *Liriogamæ*, for the petaloideous or non-glumaceous *Monocotyledones.* The fragment relates principally to Liliaceous and Amaryllideous genera, and is very curious and interesting. It must of course be insisted that future *Genera Plantarum* are not to be burdened with the synonymous names which here first see the light,—e. g. *Xeniatrum* for Rafinesque's *Clintonia*, *Neolexis* for *Smilacina*, &c. The interest of this opuscula is solely historical and critical. We could wish that the able editor had put upon record a general account of what Salisbury did for botany. A. G.

5. *Handbook of British Water-weeds or Algæ*, by Dr. JOHN EDWARD GRAY, F.R.S., &c.—*The Diatomaceæ*, by W. CARUTHERS, F.L.S., &c.—London: Hardwicke, 1864. pp. 123, 18mo.—An excellent little manual, and one which may be very useful and convenient in this country also (so many of the *Algæ* being identical), containing as it does the Fresh Water species, but these only in an arranged list, with references to the leading figures. The *Desmideæ* and *Diatomaceæ* add much to its value.

A. G.

6. *Scolopendrium officinarum in Western New York: probable determination of the original locality of Pursh*; by J. A. PAINE, JR.—At the request of Dr. Gray, a trip to the hills of south Herkimer County for rare orchids, was lately extended to Onondaga County for the identification, if possible, of the habitat of this fern, so rare with us, which Pursh discovered and recorded. The ravine of Chittenango creek is too far east by twenty miles or more to be referred to his remark. Jamesville, therefore, was visited to find out how far this new station is from "Onondaga," and if near or upon lands which ever were "plantations of J. Geddis, Esq." At once it was seen that this locality—detected last March by Mr. Lewis Foote, as announced in the May number of this Journal—though not far from Onondaga Hill, is far and nearly in an opposite direction from the residence and possessions of the late James Geddes, which are directly west of Syracuse. Mr. Foote having particularly described his station as in a rocky ravine, half a mile below the village, two hundred feet east of the railroad, &c., it was taken for granted that the place thus designated was in one of three or four points where the bed of Butternut creek narrows into rocky gorges, or at the entrance of a tributary stream, so a second observation appeared unnecessary. Attention however, was directed to two or three interesting localities known as "pit-hole lakes," deep depressions in the surface, walled round on all sides but one with rock at least one hundred feet high, a quarter of a mile across from side to side, usually having a small pond in the center with no visible outlet, localities of which no satisfactory explanation has been given, but greatly resembling whirlpools, as the one in the Niagara river. On the shaded talus of the nearest of these, "Little lake," about one mile west of the town, *Scolopendrium* was detected in limited quantity, with *Camptosorus rhizophyllus*. "Green pond" and "White lake" occur near together, two miles east of Jamesville, at the base of a remarkable outcrop of the limestone range, from one to two hundred feet high and four or five miles long, the former similar in character to Little lake, and lying far within the irregular line of the cliff, like a bay along its coast. These "highlands," before they were cleared and burned over, formed the very kind of locality where our rare fern delights to dwell, possessing all the conditions of loose limestones, rich mould, moisture and shade; and no doubt, their high rocky steeps formerly abounded with it. This presumption is confirmed by the fact that on a particular part of the range, where the fire and clearing ceased and the undisturbed forest began, on the talus of a low ledge, just there was *Scolopendrium* found growing in its greatest luxuriance and scattered along the bank for a fourth of a mile or so, as far as covered by woods. Directions to other like places by a gentleman

in the village who recognized the plant, indicate that it may not be infrequent throughout the town.

Onondaga valley affords frequent outbreaks of the same limestone rock along its sides and in gorges of streams descending to the creek, where this fern may grow.

Hon. George Geddes, son of the J. Geddes, Esq. referred to by Pursh, was then appealed to for information in general respecting this fern or its earliest station, and he readily cleared up the whole mystery. The place where it was discovered, he said, was nearly five miles west of Syracuse and half a mile south of his father's house; on the single point of its being on his father's farm Pursh must have erred; but it was near by, along a high ledge and about a celebrated sulphur spring. Mr. Geddes very kindly extended the hospitalities of the same mansion in which Pursh made his stay while exploring in this region, and accompanied the writer to a locality called Split-rock, half a mile south of Fairmount, the residence of Mr. Geddes, who confidently believes this to be the place where *Hart's-tongue* was discovered and formerly flourished. He recollects perfectly well how, when a boy, the existence of the fern having been doubted, his father charged him to search carefully for it in his hunting excursions, and directed him specially to this locality. Split-rock is another development of the limestone formation, probably one hundred and fifty feet high and over half a mile long, semicircular, with a brook at its base on whose bank is the sulphur spring. Its lofty and long rocky slope beneath the cliff, once a most favorable station for *Scolopendrium*, was long since cleared, dried up, and trodden over by cattle. *Walking-ferns* still linger, and even abound where there is any shade, but it is to be feared that all *Hart's-tongues* have perished.

In Madison county this plant may be looked for among the upper branches of Cowaselon creek east of the Chittenango valley, which pass through ravines and over falls; and around a number of pit-hole lakes westward. The station below Chittenango falls, brought to light about the year 1830 by William Cooper, Esq., which for so long time has been regarded as the only locality of this plant on our continent, therefore must have been unknown to both Pursh and Nuttall. The record of the latter, "*S. officinarum, v. v.* In the western parts of the state of New York, in the crevices of calcareous rocks, beneath the shade of the Hemlock Spruce (*Abies canadensis*), and accompanying the *Taxus canadensis* or American Yew," probably is merely a confirmation of the habitat of Pursh. His statement, "near Canandaigua, at Geddis's Farm, in a shady wood, with *Taxus canadensis*," as reported by Dr. Pickering to Dr. Torrey to have accompanied specimens in the herbarium of the Academy of Natural Science in Philadelphia, most likely was an error for near Onondaga, &c., easily made from similarity in the names, or from the indefinite extent covered by the former name at that time, 1806–1818. However, no such statement now exists in the herbarium at Philadelphia with Nuttall's specimens; and for the identity of his with the habitat of Pursh as above ascertained, we have "Geddis's farm," with both *Abies canadensis* and *Taxus canadensis* remaining in abundance near by.

The connection of *Scolopendrium* with Lake Simcoe, Canada West, as given in this Journal and repeated in a Catalogue of Oneida County Plants, has been a mistake for Owen Sound on the Georgian Bay. Here

It was discovered in 1857 by Professor William Hincks, growing plentifully on the rocks around the falls of a stream emptying into the Sound; since then it has also been observed by others in adjacent localities.

Geologically, this fern is confined to the limestones, and may be searched for wherever the Helderberg, Niagara and Trenton groups afford favorable stations.

Cambridge, June 15, 1866.

7. *Icones, Histiologicæ, oder Atlas der vergleichenden Gewebelehre; zweite Abtheilung. Der feinere Bau der höhereren Thiere.* Erstes Heft. Die Bindesubstanz der Cœlenteraten, mit x Tafeln und 13 Holzschnitten; by A. Kölliker. Leipzig, 1865.—This work is an elaborate essay upon the microscopic structure of Polyps and Acalephs, but more especially, upon the hard parts of the Halcyonoid Polyps. It is well illustrated by numerous beautifully executed plates and cuts, giving the details of the structure of sections of the axes of Gorgoniæ, &c.; the peculiar forms and structure of the calcareous spicula, observed in all Alcyonaria; the integuments of various Hydroids, etc. As a work illustrating the histology of these classes of animals it is invaluable and far beyond any preceding work on the same subject.

On pages 131 to 142 the author has given a Synopsis of the Classification of the Halcyonoid Polyps so far as known to him, and has introduced many new species and several new genera, with many important changes, based mainly upon the microscopic structure of the hard parts.

The genus *Primnoa* is maintained with its original limits, the subdivisions of Dr. J. E. Gray not being recognized. The genus *Muricea* is restricted to those forms like *M. spicifera* Lamx., while for the group having *M. placomus* Ehr. as its type, the new genus *Paramuricea* is established, with four species. The new genus *Echinogorgia*, allied to the two last mentioned, has the *Gorgonia sassapo* Esper, as its type, and includes four other species, all figured by Esper. This genus seems to be equivalent to my *Lissogorgia*,[1] proposed Feb. 1864, but which was not actually published until Oct. 1865, so that the former appears to have the precedence. The two species referred to (*E. flabellum* and *E. flexuosa* nov.) do not appear to be identical with either of those mentioned in this work. The genus *Plexaura* is restricted to forms like *P. flexuosa* Lamx., and for another group having *P. dichotoma* as its type, the genus *Plexaurella* has been established, with six species. The limits of the genus *Gorgonia* have been enlarged by the reunion of *Pterogorgia*, *Leptogorgia*, *Lophogorgia*, *Xiphigorgia*, *Rhipidogorgia*, *Hymenogorgia*, *Phyllogorgia*, *Phycogorgia*, &c., with the typical *Gorgoniæ* of M. Edw. and Haime. The genus *Erythropodium* is proposed for *Xenia carybæorum* Duch. et Mich., and this, with *Sympodium*, is placed in the family *Briariaceæ*.

The author is, however, certainly at fault in uniting *Gorgonia suberosa* Ellis, *Alcyonium plexaureum* Lamx., and *A. asbestinum* Pallas, into one species (*Br. suberosum* Dana), for, as I have previously shown,[2] they represent three very distinct species and two genera. But *Briareum palmachristi* Duch. et Mich. is probably identical with *B. asbestinum*. This is doubtless owing to the lack of specimens of *G. suberosa* Ellis. For the *Gorgonia suberosa* Esper, the new genus *Sclerogorgia* is established, which, as indicated, appears to be the same as *Suberogorgia* Gray. The *G. patula* Ellis and *G. verriculata* Esper are referred here, and a subfamily, *Sclerogorgiaceæ*, is instituted for the group.

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[1] Proceedings Boston Soc. Nat. History, x. 22, and Proc. Essex Institute, iv, 187.

[2] Bulletin of the Museum of Comp. Zoölogy, No. 3, p. 39, and Revision Polyps E. Coast U. S., p. 10.

The work is of peculiar value in systematic zoölogy because the author has had, for examination and illustration, the original specimens of Esper, as well as of Duchassaing and other writers, thus restoring to the science many species that have long been regarded as doubtful, or altogether neglected by many recent authors, although very well described and figured by Esper. A. E. V.

8. *The Anatomy and Physiology of the Vorticellidian Parasite* (*Trichodina pediculus* Ehr.) *of Hydra;* by H. James-Clark. From the Memoirs of the Boston Society of Natural History, with a lithographic plate. February, 1866.—In this paper the author has given an elaborate and very complete account of the anatomy, physiology, and habits of one of the most singular forms of ciliated Infusoria, and has thereby afforded the means of gaining a clearer knowledge of the general structure and classification of the entire group of Protozoa.

This parasite is stated to be the form mistaken by Prof. Agassiz for the free *Medusæ* of *Hydra*, of which the discovery was announced in the Proceedings of the Boston Soc. of Natural History, Nov. 1850, p. 354. The investigations upon the structure of this species has an additional interest on account of the views held by some authors that the *Vorticellidæ* are closely allied to the *Bryozoa*.

According to Prof. Clark, all previous figures of this species represent it in an abnormal or diseased state. "The *peristome* is not a closed circle as in Vorticellidæ proper, but follows the spiral course of the vibratory crown, and vanishes near the aperture of the vestibule. The *vibratory crown* consists of a single row of vibrating cilia, which winds along the margin of the spiral, dexiotropic peristome, just at the edge of the cupuliform disk, and descends thence to the left of the vestibular aperture, and entering it, plunges to the bottom of the vestibule, in an unbroken line. Neither Trichodina, nor any of the Vorticellidæ, possess a vestibular lash or bristle, and the latter is an optical illusion. The posterior truncate end of the body is margined by a well-defined annular *velum*, immediately behind which, and arising from the same basis, is a complete circle of vibrating cilia." * * * "The vestibule opens near and posterior to the cilia-crowned margin of the sunken cupuliform disk. The anus opens into the vestibule a short distance from its mouth, and on the right side. The contractile vesicle is a simple cavity, which performs its systole once in fifteen seconds." A. E. V.

9. *The Arctic Annelids: Nordiska Hafs-Annulater, af* A. J. Malmgren. Oefversigt af K. Vet.-Akad. Förhandlingar, 1865, Nos. 1, 2, 3.) Stockholm, 1865–66. 8vo, pp. 135, with 19 plates.—This most useful work on the higher Annelids of the Arctic seas, embracing collections made at Spitzbergen, Greenland, Iceland and Finmark, will prove invaluable in studying the species of our own coast. It is a monograph of the arctic Dorsibranchiate and Tubicolous Annelids, with numerous synoptical tables of the genera, and detailed descriptions of all the species and genera, whether new or previously edited. Three new

families, the *Ampharetea*, *Sabellacea*, and *Eriographidea*, are characterized. 52 new genera and 52 new species are described. The great number of genera proposed, at least give evidence of a careful study of these difficult forms, though sometimes the generic characters seem too slight. As most of these genera, with the same or closely allied species, are likely to occur on our own coast, the work will form, with the older work of Oersted, quite a complete manual of our Annelids. The descriptions are in Latin, and the work is thus rendered accessible to all. The illustrations are very full, nearly every species being figured, and though sometimes stiff, are in the main excellent. A. S. P.

10. *On Collections of Bones of recent Rattlesnakes in fissures in limestone near Howe's Cave;* by WM. A. ANTHONY. (From a letter to one of the Editors, dated Franklin, Del. Co., N. Y., Aug. 15, 1866.)—On my way to Albany last week, in company with Prof. Orton of Antioch college, I stopped at "Howe's Cave," and there learned that at a stone quarry, about a quarter of a mile distant, the workmen had struck into a fissure filled with the bones of rattlesnakes. We visited the spot, found the bones very abundant, and collected a few specimens. We thought the subject of sufficient interest to warrant further investigation, and I therefore stopped on my return from Albany, and made a large collection which I take the liberty to send to you with the following account.

The quarry in which the bones are found is on the Albany and Susquehanna railroad, about a quarter of a mile above 'Howe's Cave.' The rock is a limestone of the water-lime group, abounding in vertical fissures. One of these was opened by the workmen in the process of working the quarry. It was several feet in width but was nearly filled by loose fragments which had fallen from the top. Among these fragments are passages worn smooth by long usage and now filled with the bones of the former inhabitants.

The rock forming the sides of the fissure is in some places covered with an incrustation of carbonate of lime an inch or more in thickness. The bones are found in great quantities. Of vertebræ and ribs, I might have collected a bushel in a short time. The man in charge of the work at the quarry told us that they had found deposits of bones that required a man ten minutes to remove with a shovel, and from my own observation I have little reason to doubt his statement. These facts show that the number of individuals that inhabited the cavern must have been enormous.

11. *Mémoires pour servir a l'Histoire Naturelle du Mexico, des Antilles et des Etats-Unis*, par HENRI DE SAUSSURE; III and IV Livr., *Orthopteres—Blattoides*. 280 pp. 4to, with 2 col. pl. Geneva and Paris, 1864–65. (Paris, V. Masson & Fils.).—This important work on the Orthoptera of Mexico and the Antilles is based, as the preface states, mainly on the collections and observations of the author, H. de Saussure, but in part, also, on Mexican specimens received from Mr. Sallé; others from Cuba from F. Poey of Havana, and others from the United States, for comparison, received from Mr. Edward Norton of Connecticut. The volume before us, constituting the third and fourth parts, conclude the work. It is but a small part of the very valuable contributions to Zoology by Mr. de Saussure which have been published as the results of his Mexican explorations.

IV. ASTRONOMY AND METEOROLOGY.

1. *Observations on the Meteors of August last;* by DAVID TROWBRIDGE. (From a letter to the editors, dated Hector, N. Y., lat. 42½° N., long. 0° from Washington, Aug. 11th, 1866.)—The following is a report of my observations of meteoric phenomena at the August period, 1866.

Aug. 9.—I observed from 9 till 9.20 P.M. I saw fourteen meteors, the paths of four converged toward Cygnus, and seven toward Cassiopea, or a little below (to the east, estimated on the meridian). Nearly all left a visible train. Some moved slowly and others rapidly.

Aug. 10.—I observed from 9 till 10.15 P.M. in company, a part of the time, with three other individuals. We saw in all sixty-five meteors; I saw at least fifty of them. The paths of forty-seven of them converged toward Cassiopea. Some of them left long trains, one of which in particular, lasted (by estimation) six or seven seconds. I should judge that the average time of flight did not exceed half a second. Average length of path about 30°. One of them, which did not conform, was from one to one and one-half seconds in its flight. In general those which did not conform moved with less apparent velocity than those which did. Many of them were very small and moved with great rapidity.

Aug. 11.—I observed from 8.15 till 9 P.M. and saw eight meteors, four of which were conformable. It was partly clear a portion of the time. On each evening my attention was confined mainly to the region of the heavens surrounding Cassiopea.

If the rudeness of my observations would allow me to draw any conclusion, I should say that on the 9th the center of the region from which the meteors came was somewhat *below* the *chair* (as seen at the time of observation); on the 10th nearer the chair; and on the 11th *in* the chair.

On the evening of the 26th of July (1866), about 8½ P.M., a very bright meteor flashed out in Cygnus, and moved from east to west with great rapidity. Its path was about 30° after I saw it. Height above the northern horizon about 50°. Duration of flight from one-half to one second. It left a beautiful train. The head was red and train blue. It was certainly *below* the clouds. It passed between me and some cirro-stratus clouds, so dense as to hide ordinary stars completely. Several others that saw it said it was *below* the clouds.

2. *Observatory of Russia.*—The place of Mr. Kupffer at the head of the Central Observatory of Russia has been filled by the appointment of Prof. Kaemtz of Dorpat.

3. *Mass of Meteoric Iron in Colorado Territory.*—Prof. Henry has transmitted to the Editors a note respecting the discovery of a mass of iron in a deep gulch near Bear Creek, Colorado Territory, about twenty-five or thirty miles from Denver, and 800 or 1000 feet below the top of a steep hill. Mr. James L. Wilson, who describes it in the Daily News published at Denver, Colorado Territory, May 14th, states that it was at first mistaken by himself and Mr. G. R. Morrison who accompanied him and who had seen it before, for the 'blossom' or "iron hat" of a mineral lode. "It is irregular in form, being about twenty-two inches long, nine to ten broad, and fourteen wide. Four of its faces are flat and two rounded. This form indicates it to be a fragment of a much larger mass.

It is magnetic. Its weight is estimated at 500 pounds. The force with which it struck the rocks at the time of its fall had so shattered one end as to enable the discoverers to break off a piece that weighed eleven pounds. Its composition appears to be iron, nickel, cobalt, and copper, unequally distributed in its mass. In one part the nickel and cobalt are largely in excess of the other metals, while in other parts iron forms the chief ingredient. These metals are aggregated and highly crystallized. A coating of the oxyd of iron half an inch thick has taken the place of the shining black crust observed on aërolites when they first reach the earth. The less oxydizable metals, nickel and cobalt, still remain in their metallic state in this coating of iron rust."

It is pretty certain from this not satisfactory description, that this is an example of an iron meteor-mass found where it has fallen, the shattering of the mass and of the adjacent rocks being rarely observed. It was exposed by a freshet which had washed away the loose stones and earth. This is the same mass noticed by Prof. Shepard at page 250 of this issue, who appears to have been in possession of some scales from the concussion which disintegrated the specimen. We have taken steps to obtain more detailed information respecting it.

4. *Reduction of the Observations of Fixed Stars made by Joseph le Paute d'Agelet*, at Paris, in 1783–1785, *with a Catalogue of the corresponding mean places referred to the equinox of* 1800·0; by BENJAMIN APTHORP GOULD. 262 pp., 4to. From the Memoirs of the National Academy of Sciences, Vol. I. Washington, 1866.—This paper, which makes a worthy beginning of the Memoirs of the National Academy of Sciences, opens with some account of the astronomer d'Agelet, by whom the observations were made, a description of his instruments, explanations of the methods of reduction adopted, and remarks on other points of interest, and then proceeds with the tables of reductions. D'Agelet was appointed Professor of Mathematics at the *Ecole Militaire* in 1777. In 1785, after the last of the observations above mentioned were made, he left France as astronomer of LaPeyrouse's expedition around the world, and shared its fate, the vessels having been wrecked in 1788 (as ascertained forty years afterward) on the reefs of Malicollo, one of the New Hebrides, and all on board lost. D'Agelet had been very industrious throughout the course of the expedition, having established an astronomical observatory at each of the ports visited. But his commander did not allow him to send any of his results home, and none of them were saved.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The American Association.*—The American Association for the Advancement of Science, after a suspension of its meetings for five years in consequence of the war, held its fifteenth meeting at Buffalo, N. Y., commencing on Wednesday, August 15th, and continuing until Tuesday, the 21st.

The officers of the meeting were: Pres. F. A. P. BARNARD of Columbia College, *President;* Dr. A. A. GOULD of Boston, *Vice President;* Prof. ELIAS LOOMIS of Yale College, *General Secretary;* Prof. JOSEPH LOVERING of Harvard College, *Permanent Secretary;* Dr. A. L. ELWYN of Philadelphia, *Treasurer.*

Though there were fewer members in attendance than at some former meetings, owing partly to the shortness of the notice, and partly to pecuniary and other considerations, yet in the number and value of the papers presented, the earnestness and ability of the discussions, and the general harmony of the proceedings, this meeting was throughout eminently successful, and will compare favorably with preceding ones. It left in the minds of the members a strong conviction of the importance of the Association to the science of the country, particularly as a means of stimulating research, and promoting friendly intercourse among scientific men.

Several prominent members, unavoidably absent, expressed their regrets by letter, as well as their abiding interest in the Association. The feeling appeared to be general among the members that, hereafter, the meetings, from year to year, would be fully attended, as before the war.

The success of the meeting was very greatly promoted by the cordial coöperation and generous hospitality of the citizens of Buffalo, who not only were prepared to entertain at their homes all who came, but, by social receptions, excursions on the lake and to Niagara, and many other acts of kindness and appreciation, contributed largely to the enjoyment of the members, as well as to their own reputation for intelligence and public spirit.

The next meeting is to be held at Burlington, Vt., commencing on the 21st of August, 1867. The following officers were appointed for the year ensuing: Prof. J. S. NEWBERRY of N. Y., *President;* Prof. WOLCOTT GIBBS of Cambridge, *Vice President;* Prof. JOSEPH LOVERING of Cambridge, *Permanent Secretary;* Prof. C. S. LYMAN of New Haven, *General Secretary;* Dr. A. L. ELWYN of Philadelphia, *Treasurer.*

The following titles of the papers read are from the newspapers of Buffalo, in which quite full reports were given:

The Spots on the Sun; Prof. E. LOOMIS.
On the period of Algol; Prof. E. LOOMIS.
On the path of the meteoric fire-ball of 1860 which passed over Buffalo; Prof. COFFIN.
The Dearborn Observatory of Chicago; Prof. T, H. SAFFORD.
Mutual action of electrical currents; E. B. ELLIOTT.
New method of illuminating apparatus for opaque objects under the microscope; Pres. F. A. P. BARNARD.
Achromatic registration of meteorological phenomena; G. M. HOUGH, of Albany.
Map of Magnetic Declination; J. E. HILGARD, U. S. Coast Survey.
Effect of sunshine on fire; Prof. E. N. HORSFORD.
On the automatic barometer; G. M. HOUGH.
On the anthistometer; Dr. L. BRADLEY.
Theory of meteors; D. KIRKWOOD.
On the Aelloscope; HENRY A. CLUM.
General meteorological features of the west; Prof. O. N. STODDARD.
On fundamental Star-catalogues; Prof. T. H. SAFFORD of Chicago.
On a new method for the construction of Life and Annuity tables; E. B. ELLIOTT of Boston.
On the statistical systems of certain countries of Europe; E. B. ELLIOTT.
On Decimal weights and measures; B. S. LYMAN.
On the galvanic battery; Dr. BRADLEY.

The Geology of Southern Minnesota; Prof. JAMES HALL.
Structure of the mountains and valleys in Tennessee, Northern Georgia, and Alabama; JAMES HALL.

On the Stromatiporidæ; Prof. A. **Winchell.**

On the Rocks of Kansas; G. C. **Swallow.**

The Laurentian Limestones and their minerals; T. **Sterry Hunt.**

On the primeval atmosphere; T. **Sterry Hunt.**

On petroleum; T. **Sterry Hunt.**

On the internal structure of Athyris, Meristella, and the allied genera; **Jas. Hall.**

On the structure and mode of growth of the spines on the cardinal area of Chœtes; **James Hall.**

On Cryophyllite, a new mica; Prof. J. P. **Cooke.**

On a new chemical nomenclature; S. D. **Tillman.**

On the genesis of auriferous sulphids; H. **Wurtz.**

The Glaciers of the St. Lawrence; Col. **Whittlesey** of Cleveland.

Glacial epoch in the valley of the Mississippi; Dr. **Newberry.**

On the drift and its origin; **James Hall.**

On the drift of the Western and Southern States, and its relations to the Glacier and Iceberg theories; E. W. **Hilgard** of Mississippi.

Evidences of Glacial action in Southeastern New York; **James Hyatt** of Bengall, N. Y.

On the supposed plasticity of pudding stones; B. S. **Lyman.**

On a section of the strata in Northeastern Ohio and Western Virginia; Prof. E. B. **Andrews.**

On the origin of prairies; Dr. J. S. **Newberry.**

On steam-boiler explosions; Prof. O. N. **Stoddard.**

On the effects of alum as used in making bread; Prof. E. N. **Horsford.**

On the fruit-producing belt of Michigan; A. **Winchell.**

Proportional dimensions of the human frame; B. A. **Gould.**

A eulogy on the late President Hitchcock was delivered by Mrs. A. L. Phelps, and an address on scientific studies as a means of mental discipline, by Prof. J. P. Cooke. The Association passed a resolution in favor of the introduction into common use of the decimal system of weights and measures.

2. *Addition to Article on Method of correcting Monthly Means* (page 154); by E. L. **DeForest.**—In general, if we have any three consecutive monthly means given, and wish to infer from them, as nearly as possible, what the form of the curve must be, our knowledge respecting it comes under two heads. First, it must be a curve of three parameters; for the three given monthly means are sufficient to determine three, and only three, constants in the equation of the curve. Secondly, it ought to be a periodic curve, with an annual period; for we know that at the end of one, two, or any whole number of years, the same monthly means occur over again. Now the parabola satisfies the first of these two conditions, but not the second. The trigonometrical curve satisfies both; so that there is reason for preferring it in all cases.

3. *Flint implements.*—V. **Chatel** has recently collected some hundreds of chipped flints, of the age of Stone, in the fields of his estate of Campandré-Valcongrain and the surrounding villages. They were found at the surface, especially after ploughing. Among them there is a saw well toothed, about six centimeters long; a whistle of stone which gave out a very acute sound. On the same estate there are wooded hills which contain several hundreds of Celtic tombs, some of which, of an oval form, are five to seven meters long and project above the surface more than a meter. There are also large blocks of stone, said to be Druidic, which Mr. Chatel regards as ancient altars, and which may belong to the age of Stone.—*Les Mondes*, p. 137, May 24.

4. *Library of works on Earthquakes and Volcanoes of Prof. Alexis Perrey.*—Professor Perrey, of Dijon, has recently offered for sale his very extensive library—probably the best on the two topics of Earthquakes and Volcanoes in Europe. The catalogue of it which has been published shows that it contains 4015 works, including pamphlets. It would be an exceedingly valuable acquisition to any of the larger American libraries, and we hope that it may not fail to be secured to the country. Professor Perrey has been a long and earnest laborer in his favorite departments of science and has spared no pains to make his library a complete one.

OBITUARY.

Prof. JOHN A. PORTER.—John Addison Porter died at New Haven, Conn., on the 25th of August. Prof. Porter was born in Catskill, N. Y., March 15th, 1823, and graduated at Yale College in 1842. Possessed of literary as well as scientific tastes, he was called to fill the post first of tutor, and then of Professor of Rhetoric, in Delaware College, in Newark, N. J., where he resided from 1844 to 1847, when he went to Germany and studied chemistry under Prof. Liebig. Returning in 1850, he first filled the Professorship of Chemistry applied to the Arts in Brown University for two years, when he was called to take the place of Prof. John P. Norton, then recently deceased, as Professor of Analytical and Agricultural Chemistry in Yale College. In 1856 he was transferred to the chair of Organic Chemistry, which position he held until he felt constrained by his declining health to resign it, in 1864.

He then spent a year in Europe, to avail himself of the best medical advice, and returned less than a year ago, apparently greatly benefitted in health. It was soon evident, however, that his disease was not wholly eradicated, and he has at length, after months of intense suffering, during which he exhibited a remarkable degree of fortitude and Christian resignation, gone to his final rest.

In connection with the Sheffield Scientific School, the activity and zeal of Prof. Porter enabled him to do excellent service, both for the institution and the cause of agricultural science throughout the United States. He was chiefly instrumental in originating and conducting the very successful course of Agricultural lectures, which, in 1860, attracted large numbers of persons to New Haven from distant parts of this country. In the reorganization of the School, about the same time, he took an active part, and some important changes were largely due to his forecast and energy.

Prof. Porter was a ready and forcible public speaker, with a clear and flexible voice and a fine personal appearance. His labors in behalf of the Sanitary Commission and in promoting in various ways the cause of his country, were particularly earnest and effective, so long as his failing health permitted him to work. He was ever zealous for truth and justice, and in carrying out his plans of usefulness, exhibited great fertility of invention, and the most unwearied assiduity.

He is the author of a text-book on Chemistry, which has gone through many editions.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Geological Survey of Illinois:* A. H. WORTHEN, Director. Volume I, Geology. xvi, and 504 pp. large 8vo, with maps and sections. 1866. Published by the authority of the Legislature of Illinois.—This first volume of the Geological Survey of Illinois, by its able Director, Mr. Worthen, has just been issued, and so near the publication day of these pages that we can at this time barely announce its appearance. The volume is full of matter of great interest to American geology and to science in general, and at the same time it must meet the demands of those at home who have looked to the State Geologist for an exposition of the mineral resources of the State. The various subjects are well treated, and evidently as the result of extended and faithful exploration. The style of publication is every way handsome and generous. Mr. Worthen has had the assistance of Prof. J. D. Whitney in the survey of the lead region; of Prof. Leo Lesquereux in that of the Coal formation and the subject of prairies; and of Mr. Henry Engelmann in the general field work and chemistry. The volume will be followed by another on the Paleontological part of the survey, in the preparation of which Mr. Worthen has been aided by Mr. F. B. Meek, Dr. J. S. Newberry and Prof. Lesquereux.

This notice of the Geological volume is but an introduction to an abstract of its contents which we propose to give in another number.

2. *Reliquiæ Aquitanicæ*, being Contributions to the Archæology and Palæontology of Périgord and the adjoining Provinces of Southern France; by EDOUARD LARTET and HENRY CHRISTY. Parts I and II, Dec. 1865 and March 1866, each 24 pages 4to, with 6 lithographic plates. London. (H. Bailliere. Each part 3*s.* 6.)—The *Reliquiæ Aquitanicæ*, or Aquitanian remains, are flint implements and the bones associated with them in Southern France (the region being part of the Aquitania of the Romans); and the work upon them bearing the above title treats of these implements and bones, and the bearing of the facts relating to them on European Archæology. As the preface states, the work was projected by Mr. Christy; and its first sheets were already in the press, and some of the plates engraved, when he died after a brief illness brought on by over-exertion in a visit to the Belgian bone-caves. The principal labor of preparing the work was thus thrown upon his fellow-worker, Mr. Lartet, who is now aided by Mr. P. l'Haridon of France, and Messrs. John Evans, A. W. Franks, W. Tipping and Prof. T. Rupert Jones, the last-mentioned taking the editorial duties. The work is issued in elegant style, both as to text and plates, and treats of one of the most interesting subjects in modern science. The figures give admirable views of the flint arrow-heads, chippings, etc.; of the bones, part of them carved or etched with figures of different animals upon them; and of the arrow heads, harpoon heads, etc., made of reindeer's horn. It is to be completed in about 20 parts of 24 pages and 6 plates each.

Baird's Review of American Birds.—We have received sheets 21 to 28 inclusive (pp. 321-450) of Professor Baird's work.

Geological Map of England and Wales, by Prof. A. C. RAMSAY, F.R.S. 3d edition. London. [Stanford.]

On the Anatomy of Vertebrates; vol. II. Birds and Mammals; by R. OWEN, F.R.S. London. [Longmans.]

The British Pleistocene Mammalia; by W. Boyd Dawkins, Esq., M.A., F.G.S., and W. Ayshford Sanford, F.G.S. Part 1. Published by the Palæontological Society.

The Anthropological Treatises of Johann Friedrich Blumenbach, with Memoirs of him by Marx and Flourens, etc., translated by T. Bendysche, M.A. Published for the Anthropological Society. [Longmans.]

Histoire des Crustacés fossiles; par E. Blanchard. The first volume of this work on Fossil Crustacea was presented by the author to the Academy of Sciences in April last.

Animaux fossiles et Géologie de l'Attique, d'après les recherches faites par M. Albert Gaudry. Paris. (E. Lévy.)—The 13th (last) part of this important work has been issued. The whole makes a volume in small folio of 323 pages, with 62 lithographic plates.

Clavis der Silicate, von Dr. Leop. Heinrich Fischer. 114 pp. 4to. Leipzig 1864. (W. Engelmann.)—Consists of tables for the determination of the mineral silicates.

Jahresbericht des Vereins für Erdkunde zu Dresden. Dresden, 1865. Erster, 30 pp. 8vo; Zweiter, 57 pp. 8vo, with also a paper of 24 pp. entitled Der Chaldäer Seleukos; eine kritische Untersuchung aus der Geschichte der Geographie, von Dr. Sophus Ruge.

Archiv für Anthropologie. Zeitschrift für Naturgeschichte und Urgeschichte des Menschen; by C. E. v. Baer of St. Petersburgh, E. Desor of Neufchatel, A. Ecker of Freiberg, W. His of Basel, L. Lindenschmit of Mainz, G. Lucae of Frankfurt a. M., L. Rütimeyer of Basel, H. Schaaffhaussen of Bonn, C. Vogt of Geneva and H. Welcker of Halle, under the special direction of A. Ecker and L. Lindenschmit. Appears irregularly in parts, 3 of which make a volume. Published by Friederich Vieweg & Sohn, at Braunschweig.

Proceedings Acad. Sci. Philadelphia, No. 2. April, May, 1866.—Page 101, History of the "small black erratic Ant"; *G. Lincecum.*—p. 110, Notes on some members of the Feldspar family; *I. Lea.*—p. 113, On Chætetes and some related genera, with descriptions of species; *C. Rominger.*—p. 123, 4th Contribution to the Herpetology of Tropical America; *E. D. Cope.*—p. 133, On 5 n. sp. of Unio, and 2 of Lithasia; *I. Lea.*—p. 134, Critical review of the Procellariidæ, Parts IV and V, with a general supplement; *E. Coues.*—p. 197, On the cranial forms of the American Aborigines; *J. A. Meigs.*

Proc. Bost. Soc. Nat. Hist., Vol. X.—p. 296, On the modifications of oceanic currents in successive geological periods; *N. S. Shaler.*—p. 302, relations of the life of individuals among tetrabranchiate Cephalopods, and the collective geological life of the same; *A. Hyatt.*—p. 305, On a mineral resembling Albertite from Colorado; *W. Denton, A. A. Hayes.*—p. 309, New species of Schiedea, and an allied genus; *H. Mann.*—p. 320, Chemical analyses of minerals associated with the emery of Chester, Mass.; *C. T. Jackson.*—p. 323, On Polyps and Corals of Panama, with descriptions of new species; *A. E. Verrill.*

Annals of the Lyceum Nat. Hist. of New York. Vol. VIII, Nos. 8—12.—Page 213, Notes on Species of the family Corbiculidæ, with figs.; *T. Prime.*—p. 238, Summary of the meteorological register for New York in 1865; *O. W. Morris.*—p. 240, Embryology of Star fishes; *A. Agassiz.*—p. 247, Examination of American Blendes for Thallium and Indium; *C. A. Joy.*—p. 251, Geology of Sombrero; *A. A. Julien.*—p. 279, Catalogue of Birds observed in New York, Long and Staten Islands and vicinity; *G. N. Lawrence.*—p. 301, New species of Reptilian bird from tracks in the Trias of Massachusetts; *C. H. Hitchcock.*—p. 303, On the Young stages of a few Annelids; *A. Agassiz.*—p. 350, Seven new species of Birds from Central and South America; *G. N. Lawrence.*

Proc. Essex Institute, Salem. Vol. V, No. 1. Jan., Feb., Mar., 1866.—Page 3, Prodrome of a monograph of the Pinnipedes; *T. Gill.*—p. 14, Notice of a Foray of a colony of *Formica sanguinea* on a colony of a black species of Formica; *J. A. Allen.*—p. 17, Synopsis of the Polyps and Corals of the N. Pacific Exploring Expedition; *A. E. Verrill.*—The Naturalists' Directory, Part II, 16 pp. Appendix to the Naturalists' Directory. The object of this Appendix (which is to be continued), is to facilitate naturalists in procuring or disposing of specimens by exchange or otherwise, and to give changes of address, additions of names to the Directory, and notices of other matters of interest. *Five lines* in it are allowed to each subscriber to the Proceedings for notices of specimens for sale, and additional space at the rate of 10 *cents a line.*

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. XLI.—*William Rowan Hamilton.*[1]

WILLIAM ROWAN HAMILTON, one of the ablest mathematicians that this or any other country has produced, and for nearly forty years a fellow of the Royal Astronomical Society, was born in Dominick Street, Dublin, in the year 1805. His father was by profession an attorney, and was long held in great estimation both for his personal character and his professional ability. The branch of the Hamilton family from which he was descended originally settled in the north of Ireland, in the reign of James the First; and it is said that by right a baronetcy belonged to the representative of this branch, a near relative of his own; although the claim could not be fully supported, owing to merely technical flaws. Thus Hamilton may have been in some degree indebted for his great and versatile mental capacity to a mixture of race.

William Hamilton is one of those rare instances, where the promise of early childish precocity has not been disappointed by the attenuated achievements of riper years. At various stages of his boyhood, not to say childhood, for the precocity manifested itself at the early age of *four*, he is said to have successively acquired some notable acquaintance with no less than thirteen languages, European and Asiatic. His attention was directed to the latter, because it was originally hoped that, enjoying as he did the opportunity of good patronage, his career would be passed in India. It is recorded on evidence which deserves respect, that at the age of seven he was examined in Hebrew by a Fellow of Trinity College, Dublin, and that "the child passed a better examination in that language than many candidates for the fellowship." For obvious reasons we hope there is some pardonable though very natural exaggeration in the statement. It is certain however that the attention of the Persian Ambassador, when on a visit to Dublin, was attracted by a letter of greeting written in Persian by young Hamilton at the age of fourteen. Whether or not any allowance is to be made for the shadow of the future overlapping the memory of the past, it is quite certain that the vast intellectual capacities of the boy were evinced and cultivated at a very early age, and what is of far greater consequence, this early mental activity did not prostrate or forestal the successful exertions of maturer life. It is quite possible that the literary turn thus given to his earlier pursuits may have happily laid the foundation of that peculiar combination of metaphysics and poetry, which distinguished some of his mathematical performances from those of most other men. For his early training in ancient and modern languages, he was indebted to the loyal care of his uncle, the Rev. James Hamilton, curate of Trim; but in science and mathematics he appears to have been nearly self-taught and self-directed; in his case, as in that of many other eminent men, this circumstance probably conduced to the originality of his maturer conceptions, and to the peculiar style in which he embodied them.

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[1] From the Monthly Notices of the Royal Astron. Soc., xxvi, 109.

By the age of fifteen, young Hamilton had mastered the usual course of elementary mathematics, pure and applied; and in some instances had become familiar with works of original research. He appears to have evinced a peculiar taste for long and difficult arithmetical approximations, and to have shown himself no mean antagonist in the solution of numerical puzzles when matched against a certain arithmetical prodigy, who, coming from America, happened at that time to be exhibited in Dublin. By the age of seventeen he had mastered Newton's *Principia*, and a year later found him in possession of most of the processes in the *Mécanique Céleste.* Meanwhile, and notwithstanding this very unusual advancement in mathematical knowledge, the main culture of his mind had been classical; and that, not alone from natural predilection, but on account of the requirements of the collegiate course on which it was his intention to embark and to compete.

It is almost needless to say that young Hamilton, with a mind thus disciplined and furnished, entered upon his course at Trinity College, Dublin, if not without able competitors, at all events without an equal, whether in literature or mathematics. As might be expected, he carried all before him; and when we speak of success in his literary efforts, it must be understood that we include Poetry in the list, inasmuch as on two successive occasions he gained the Vice-Chancellor's Prize for English verse. It is to this early and successful cultivation of the lighter elegancies of scholarship that his friends were indebted for a vein of poetical thought and expression which graced alike his correspondence and his conversation, and which is sometimes observable even in his graver compositions.

It appears that in the year 1822, one year before his entrance at the University, young Hamilton, now in his eighteenth year, attracted the notice of the celebrated Dr. Brinkley, by certain objections which he made to a demonstration propounded by Laplace in the *Mécanique Céleste.* On being invited to pay a visit to that well-known astronomer, the young student thought that he should most properly express his feelings of respect by carrying in his hands another instance of independent research on the osculation of certain curves of double curvature. This introduction of Hamilton to the veteran professor laid the foundation of a mutual friendship and respect which continued to increase during Dr. Brinkley's tenure of office.

In the first year of his student life at Dublin, Hamilton, notwithstanding his close attention to the elementary line of study necessarily prescribed to undergraduates, nevertheless engaged himself in a line of original research. Even before his entrance at the University he had directed his thoughts to the difficult subjects of Caustics, and having now completed the memoir, it was read before the Royal Irish Academy in 1824. This paper was referred as usual to the consideration of a committee of scientific men, who being struck with the originality of the conception, and the evidences of analytical power which it contained, recommended the author to give those further developments of the subject which evidently lay within his grasp. The result of this encouragement to the young philosopher was the speedy completion of a memoir which may be said to contain the germ of a large portion of the noble work which it was his lot to contribute toward the advancement of physical knowledge. Instead of an essay on Caustics, his paper was now enlarged into a wider and more general investigation, under the title of a "Theory of Systems of Rays." It may be no exaggeration to say of this memoir, in conjunction with its subsequent supplements, that it is one of the ablest contributions ever made to our knowledge of the geometry of optics. Chasles, one of the most distinguished of modern geometers, speaks of it as "*dominant toute cette vaste théorie.*" Starting from the simple fundamental principle that light, whatever may be its cause or its constitution, is amenable to what mathematicians call "The Principle of Least Action," or, in other words, probably as true, and certainly more expressive, amenable to the *principle of no waste* *in nature*, Hamilton, in a train of analytical logic unimpeachable, and with a mastery over the management of algebraic symbols probably never surpassed, shows that the theory of a system of rays reflected or refracted any number of times at given surfaces, depends on the determination of a single principal function V, which contains in itself all the properties of the system of rays, in a manner analogous to that in which the properties of a curve are contained in its equation. The same theory is, in the supplements, extended to the more complicated and recondite question of double refraction in biaxial crystals, and at length lands the reader in one of the most remarkable scientific predictions contained in the records of physical inquiry. But of this prediction we must speak presently.

When the first part of this "Theory of Systems of Rays" was presented in April 1827 to the Royal Irish Academy, it will be remembered that Hamilton was as yet an undergraduate of twenty-one years of age. In this year the Professorship of Astronomy in Trinity College, Dublin, became vacant by the promotion of Dr. Brinkley to the bishopric of Cloyne. Such was his deserved reputation, that, notwithstanding the appearance of other and most formidable candidates in the field, and although, moreover, he had as yet taken no academical degree, Hamilton was elected to the vacant chair.

This circumstance is of itself sufficiently remarkable, and reflects equal honor upon the authorities who ventured to make the appointment, and on the young geometer who, by dint of genius and laborious study, was qualified to discharge the duties of the post. In connexion with this arrangement there is a point of osculation with our own Society of sufficient interest to demand our notice. The present Astronomer Royal, at that time Lucasian Professor of Mathematics in the University of Cambridge, was one of the candidates for the vacant post at Dublin; and he too, like Hamilton, had been advanced to his professorship before he had ceased nominally to be in pupilage. We are not here, even by the remotest implication, suggesting a comparison between these eminent men; such a comparison would not only be utterly unfitting, but, owing to the divergence of the lines of research adopted by the two geometers, would be wholly impossible. Nevertheless the thought unavoidably presents itself, that for both parties, and for the general interests of science, the decision of the Dublin electors was a happy one. Had it been otherwise, the one, in all probability, from certain natural tendencies of his mind, would have become a clergyman—no doubt a most eminent one—in the Irish Church; while Greenwich and our own Society might have lost the other.

> "There is a Divinity which shapes our ends,
> Rough hew them how we will."

In 1828 Hamilton became a Fellow of the Royal Astronomical Society, and thus at the time of his decease was among the oldest, as his name was certainly among the most honored, of our members. In 1833 he made known, in one of several supplements to the "Theory of Systems of Rays," his great discovery of Conical Refraction. In this memoir, starting again from the principle of least action, and, as before, conducting the investigation by means of a single Principal Function, he establishes the entire theory of double refraction; and, applying it to the case of biaxial crystals, by a new and simpler method[1] than that originally pursued by Fresnel, he obtains the equation to the form of the wave assumed by the vibrating ether within the crystal. On examining the form of the wave surface, Hamilton, with remarkable sagacity, observed that if the theory and the results were true, a single ray of light incident at a certain angle on a biaxial crystal, must of necessity pass into it, not as one ray, nor even as two rays, but as a conical sheet of light, and then finally emerge as a luminous cylindrical surface. And, again, his profound and complicated analysis indicated that there was also a direction within the crystal, such, that if an internal ray of light passed along it, it would emerge from the crystal, not as one ray, but as a luminous conical shell. Such results as these were not only apparently contrary to all analogy and expectation, but formed, if the experiment could indeed be made, a species of *experimentum crucis* of the truth of the undulatory theory of light. Notwithstanding the difficulty of the case, the experiment was at length successfully performed by Dr. Humphrey Lloyd, of Dublin, whose patient ingenuity, and faith in the profound work of the geometer, were rewarded by the sight, for the first time, of what cannot properly be called less than the astonishing phenomenon of a single ray spread out, by refraction in a crystal, into an infinite number of rays, forming the surface of a luminous cone.

From the sagacity of Hamilton and his friend Dr. Lloyd, thus constraining the little crystal of Aragonite to give up, Sphinx-like, its secret of ages, our thoughts unavoidably turn to the parallel case of Adams and Leverrier, who, from a similar strong faith in the laws of nature and in the logic of geometry, not only predicted the existence of a planet heretofore unseen and unexpected, but indicated the precise region of the heavens, where, as soon as looked for, it was actually found. We do not regard such results as valuable only because they corroborate our conviction of the existence of certain laws whereon we believe the universe to have been constructed by the Author of Nature, but still more so because they serve to encourage the student to persevere in his researches, animated by the fullest conviction that if truthfully conducted they can only land him in truth, and leaving the *cui bono* to be determined by the appreciations, or the wants, or the curiosities, of men in time to come.

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[1] It is but a point of justice to state that Mr. Archibald Smith has since much improved the simplicity of the process by a very elegant method of elimination.

The Royal Irish Academy took cognizance of Hamilton's great discovery, and of the profound mathematical skill whereby it was evolved, by conferring upon him their Cunninghame medal; and the Royal Society awarded him a similar mark of their appreciation of his merits. In 1837 he was elected President of the Royal Irish Academy, succeeding[a] his friend and early patron, Dr. Brinkley, in the chair, as he had succeeded him in the Professorship of Astronomy. He retained this distinguished office for eight years, and on his resignation he received the thanks of that eminent Academy "for his high and impartial bearing in the chair."

In 1834 and 1835 he communicated to the Royal Society two papers on "A General Method in Dynamics." Here, again, he commenced with the same fundamental idea, as that which he had already so successfully adopted in his "Theory of Systems of Rays," and he showed that the integration of the differential equations of motion for any system of bodies may be considered as depending on the determination of a certain Principal Function, which he defines in several different forms, but in each case by means of *two* partial differential equations involving, one of them, the differential coefficients in regard to the final coördinates, (coördinates at the time t), the other, those in regard to the initial coördinates of the several particles. He also established in these memoirs the now well-known "*Hamiltonian Form*" of the equations of motion of any material system.

The two Memoirs just referred to gave occasion to Jacobi's investigations on "Partial Differential Equations" (*Crelle*, t. xvii, 1837). Jacobi shows that, instead of "Hamilton's Function" involving the time and the initial and final coördinates, and satisfying *two* partial differential equations, it is allowable to consider a function of the time and the final coördinates only, satisfying a *single* partial differential equation: and he considers that by omitting to make this simplification, Hamilton presented his remarkable discovery in at least an imperfect light. There can be no doubt that the simplification thus introduced by Jacobi was a most important and valuable one; but it can scarcely be objected to Hamilton that he failed to perceive all the results deducible from his own discovery, any more than it can be objected to Fresnel that he left it to Hamilton to deduce conical refraction from the very form of the wave surface which Fresnel was the first to investigate. It must not be forgotten that it is to Hamilton's discovery as their fountain, though the course of the stream was directed by Jacobi, that are due all the developments which have since been made in the vast subject of Theoretical Dynamics. In a word, it may not be too much to say that the step in advance made by Hamilton's two memoirs can only be compared with that effected at an earlier epoch by the publication of Lagrange's *Mécanique Analytique.* For this work, also, Hamilton was again awarded a gold medal by the Royal Society.

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[a] Dr. Lloyd, sen., was President for two years after the death of the Bishop of Cloyne. Hamilton succeeded Lloyd.

We pass over various other characteristic works of this profound analyst, not because they are devoid of interest or of worth, but because they are less within the scope of our Society; and we come at lengh to what Hamilton considered the crowning labor of his life,—a labor which for the next twenty years, and indeed till within a few days of his decease, continued to occupy his thoughts. The labor here referred to was bestowed on the invention and the development of the Calculus of Quaternions. In a memoir such as this, and for the purposes which we have in view, we must almost despair of explaining, or perhaps of even conveying an idea of what is the aim and scope of the Calculus of Quaternions, or in fact what a Quaternion is; and yet without some such attempt, successful or not, any obituary notice of this great man would be incomplete. For this purpose, then, we must bear in mind that, in the method of geometry introduced by Descartes, and which has been retained in astronomical and physical investigations up to the present time, the position of a point in space has been determined either by its distances from three coördinate planes, or by what in reality are their equivalents. Hamilton, however, starts at once by considering not so much the position of a *point,* as rather the relation which exists between two *lines* intersecting in space, having regard both to length and to position. It will soon be seen that in order to determine these relations completely, four quantities, or four elements, are necessarily involved.

1. There is the relation which the length of the one line bears to the length of the other line;
2. The angle through which the one line must be conceived to be turned in order that it may coincide with the direction of the other;
3. The plane in which the two lines lie.

And inasmuch as the determination of this plane involves two elements, viz: 1st, its inclination to some fixed or known plane, and 2d, an element which is analogous to the longitude of a planet's node, it follows that four[1] elements or symbols are required to determine the relation which one line in space bears to another line.[4] The combination of these four elements, then, forms the Quaternion of Sir William Hamilton; and as handled and developed by him, these combinations unquestionably form a calculus of amazing generality, grasp and power. As an engine of investigation, in the general problem of combined rotations, the method of Quaternions probably has no rival in completeness or in facility. They remind one of the tentacles of some gigantic polype ramifying out into immensity, and bringing back with them the spoils of space.[5]

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[1] The above is in fact one of Hamilton's many illustrations of the meaning of a quarternion. Analytically speaking, a quarternion is an expression of the form $w+ix+jy+kz$, where i, j, k are imaginary roots of $\sqrt{-1}$, differing from the

It is as yet premature to anticipate on which of his investigations or discoveries Hamilton's fame will ultimately rest. There are mathematicians among us who in this respect would be inclined to name his Calculus of Quarternions; others would say that none of his writings can overshadow the importance of his *Dynamical Theorems.* As yet, however, the former calculus can hardly be said to be fully developed, or to have been extensively applied by other philosophers to new lines of investigation; nevertheless, it can scarcely be supposed that the persistent and conscientious labor of such a man for twenty-two successive years can fail to be full of the seeds of thought, and one day be found to admit and to invite important applications. It must however be conceded that (partly perhaps on account of its comparative novelty, and partly on account of the metaphysical atmosphere which surrounds it), the method is neither easy nor attractive to any but the ablest and most daring of the analysts among us; many a man who has essayed to bend this bow has probably said to himself what Antinous said to his boon companions:—

> "Thou wast not born to bend
> The unpliant bow, or to direct the shaft."

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imaginaries of ordinary algebra, in that the *order* of multiplication of these symbols is material, ij here not being $= ji$ but $= -ji$, and so for the other symbols.

The geometrical interpretation is this: on taking the usual three rectangular coördinate axes of x, y, z; if ij means rotate the axis of (y) round the axes of (x) through 90° of right-handed rotation, then ji must mean rotate the axis of (x) round the axis of (y) through 90° of right-handed rotation. Now the result of the former rotation is a line in the direction of the axis of $+z$; the result of the latter rotation is a line in the direction of the axis of $-z$; in this sense then $ij = -ji$ and so $jk = -kj$ and $ik = -ki$. The symbol (w) is the ratio of the lengths of two intersecting lines (or *vectors*) considered in the quaternion. Such is the first glimpse of this intricate Calculus.

[4] *Elements of Quaternions*, Longmans, 1866, page 110. This extraordinary work is the result of the unceasing labors of the last two years of Sir William Hamilton's life; indeed it is said to have been fatally injurious to his health. It was all but finished when the lamented death of the author arrested its entire completion. The Board of Trinity College, Dublin, have marked their sense of the value of this book by defraying the expenses of its publication.

[5] With this simile Sir W. Hamilton expressed his acquiescence to the writer of this memoir.

We have just spoken of the metaphysical atmosphere which seems to pervade Hamilton's Calculus of Quarternions; and herein there is little to excite our surprise, for it was natural for a man possessed of a mind so versatile and so profound, to turn it inward on itself; hence he delighted in metaphysics. But it was not alone because the culture and bias of his mind unavoidably led him in this direction, that many of his mathematical investigations assumed a metaphysical turn, but because he, in conjunction with other thoughtful philosophers, believed that no further great advance in mathematical science was now to be expected, excepting from the metaphysical point of view. Probably it is either a conscious conviction, or an intuitive perception of this, which influences the peculiar phase observable in the mathematical investigations of some of our greatest analysts of the present day.

Hamilton was not only a great mathematician, but by nature he was also a poet. He was heard to say, "I *live* by mathematics, but I *am* a poet." If, by this aphorism, he meant that, had he so chosen, he would have become more eminent as a poet than he is as an analyst, bystanders might hesitate to give their assent. Few men, perhaps, are fully conscious of the ruling bias and the strong points of their own minds. We know one of our greatest living philosophers who would perhaps say, "By filial duty I am an astronomer, but I was born a chemist." Of another it has been often said, "He *is* a mathematician and an observer, but he *was born* an engineer." Nevertheless Hamilton was a true poet, and by no means an indifferent writer of true poetry; and it is quite certain, that some of our subtlest mathematicians are poets at heart, knowing it and feeling it. And here it may be worth a passing remark to mention that Hamilton, in his great memoir on *A General Method in Dynamics* speaks of Lagrange's *Mécanique Analytique* as a *Poem.* One of our chief living Astronomers hereon remarks: "Hamilton was right, but he might have said a poem of most stately rhythm." The two works of Lagrange and Hamilton have points in common.

Hamilton counted among his friends, Coleridge, Southey, Mrs. Hemans, and Wordsworth. It is said that when Wordsworth through Hamilton's enthusiasm, was enabled to get a glimpse of the inexpressible fascination which surrounds the daring and *creative* spirit of modern geometry, the old man was for the first time inclined to admit even a mathematician into the charmed circle of the brotherhood of poets. The anecdote rests upon unquestionable authority; nevertheless we are inclined to think better things of so great and profound a mind as that of Wordsworth, and we are convinced that he must, by sheer dint of sympathy with other minds, have had at least a suspicion of the fact before the great analyst revealed it. In vindication of the justness of these remarks on the expansiveness of great intellects, and on the poetic power which almost invariably is, at the least, latent within them, we cannot refrain from quoting the following sonnet, written by a great Astronomer, on the occasion of a visit to Ely Cathedral, in company with Sir William Hamilton:—

Sunday, July 29, 1845.

The organ's swell was hushed,—but soft and low
An echo more than music rang,—where he,
The doubly-gifted, poured forth whisp'ringly,
High-wrought and rich, his heart's exuberant flow,
Beneath that vast and vaulted canopy.
Plunging anon into the fathomless sea
Of thought, he dived where rarer treasures grow,
Gems of an unsunned warmth, and deeper glow.

Oh! born for either sphere, whose soul can thrill
With all that Pöesy has soft or bright,
Or wield the sceptre of the sage at will,
(That mighty mace[7] which bursts its way to light),
Soar as thou wilt, or plunge,—thy ardent mind
Darts on—but cannot leave our love behind.

This memoir would be incomplete if we did not add, that our deceased member, together with the character of a scholar, a poet, a metaphysician, and a great analyst, combined that of a kind-hearted, simple-minded Christian gentleman; we say the latter because Sir William Hamilton was too sincere a man ever to disguise, though too diffident to obtrude, his profound conviction of the truth of revealed religion. Endued with such qualities as these, what wonder, if of his friends he was almost the idol, and of his university the pride; for he was gentle, and he was eloquent, and he spoke evil of no man, he defended the fair fame of the absent, and he held controversy with none.

Such then is an imperfect but unexaggerated sketch of this remarkable man. We will only add, that happily he did not live to survive himself, but in full possession of his faculties, almost in the very presence of the friends who had long admired him; and, what was no new thing to him, supported by the convictions and consolations of his faith, he resigned himself to his rest, as one who knew that he had done a work which had been given him to do. C. P.[8]

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[7] The symbolic analysis of which the eminent and excellent individual (Sir W. R. H.) supposed to be addressed, has proved himself a most consummate master.—(*Essays by Sir John Herschel.*)

[8] In the preparation of this *éloge*, the writer has received much assistance from Dean Graves, P.R.I.A.; the Rev. R. P. Graves, of Dublin; and Professors De Morgan and Cayley.

ART. XLII.—*The Vowel Elements in Speech;* by SAMUEL PORTER, of Hartford, Conn.

[Concluded from page 189.]

THERE are certain *modes of action of the organs* in vowel utterance, which are to be noticed as the ground of some important properties and relations. It is observable that the open vowels (deg. 3), *i*t, *e*nd, *a*t, *u*p, &c., tend, in general, to a quick, abrupt, explosive utterance,—the *i*, *é*, *e*, *ä*, and *ö* especially, and the others more or less,—except the *a* group, in which the same is true of the close vowel: they can be prolonged only by a considerable and rather unnatural effort, and then with abated force and a tendency to unsteadiness. The reason, as I conceive, is that they are formed by a neutral position of the tongue, neither drawn up toward the palate nor depressed from it: a position into which the tongue can be quickly thrown or jerked,—like thrusting an arm straight out from the body,—with firmness enough to serve for the instant for a strong vowel utterance. The reason holds peculiarly in the five anterior groups, in which the effect is most marked,—and as for the close *a*, its relationship to the open *ä* by similar position of the tongue has been already noticed.

As a matter of fact, one of these vowels is never lengthened without changing quality and becoming really another vowel: usually, either falling into the open-depressed degree,—as in the French tête, fête, from Latin testa, festa,—or sliding to a group just behind and to a closer degree,—as the prolongation of m*e*t may naturally give us m*é*re, c*a*re. In the latter case, the tongue rises just back of the terminus of the vowel-tube and thus establishes a new terminus; in either case, the operation is perfectly natural on mechanical and physiological principles. The vowel in t*ê*te, &c., is unquestionably such as to be accounted for by one or the other of these processes; and the like is true of près, accès, &c.

The middle and close vowels, on the other hand, are incapable of the same abrupt, explosive quality; and, when prolonged, usually tend to become more close, or, when at the closest, to move forward into a contiguous or otherwise related vowel of another group. These effects, again, we ascribe to the peculiar mode of action of the tongue, as, after coming into line for the group, it has to be raised to the proper degree of closeness: it is like raising the arm a little way after extending it. This motion cannot well be suddenly and firmly arrested so as to produce an abrupt or explosive utterance. It is also more natural to continue this motion than to hold it arrested so as to prolong the vowel unchanged. Obviously, also, the effect of continuing the impulse, after reaching the closest degree, would be to raise, or bend up, the tongue at a point further forward, and so to carry the vowel into another group. The middle may, however, sometimes take the course of the open degree, and move a step backward in becoming more close when prolonged.

The tendency of the open-depressed vowels, when prolonged, is, for like reasons, in the opposite direction: they incline to greater openness, so far as possible, or else to a backward movement. Thus, self, ten, &c. drawled into the open-depressed degree, incline to the *ä* if still further prolonged.

It is to be remarked, that the turn taken by vowels under change of quantity will be much influenced by the character of consonants succeeding.

These physiological actions and tendencies are important in their bearing upon vowel change in etymology, and as explaining the rationale of diphthongs and all compound vowel sounds. This will presently be illustrated by examples.

There are *relations of easy transition* between vowels of different groups,—fundamentally important as concerns the same matters just mentioned. These relations are not wholly determined by local position in the scale and on the diagram. It is in this as in geography. Localities contiguous on the map may, we know, be separated by impassable mountains, and others widely distant be in virtual proximity as united by channels of easy communication. Along with our map of the places of articulation, we need to take into view all the circumstances on which the relations among the several vowels depend.

A number of different series may be made out, founded on relations of easy transition. I will just indicate the most important, in a necessarily somewhat indefinite way, and merely in order to show that the principles I have laid down are the true physiological ground of established facts in the history of word-transformation. A full development of the application of the system would require that the original explorer should take it along with him into the field of philological research.

The two series, *a*, *ä*, *e*, *é*, *i*, and *a*, *å*, *o*, *u*, are of primary importance in philology. The *a*, *i*, and *u* are the primitive vowels of the Sanskrít and of the old Gothic, out of which the *e* and *o* were next developed,—the *ä* and *å*, as well as the *é*, not yet having a distinctly recognized separate existence. As founded on the order and manner of development in the Indo-European languages,—and the like appears, in fact, in the Semitic tongues, —we have the scheme of Jacob Grimm, with *a* at the apex of a triangle, *i* and *u* at the lower angles, and *e* and *o* respectively intermediate on the two sides. That is, we have the two series *a*, *e*, *i* and *a*, *o*, *u*. Physiologically, the first series moves forward on the line of the tongue, from the common point of departure *a*,—through the relationship before pointed out between the *a* and *ä* groups; the other moves upward along the *velum palati*,—the position of the organs for *u* marking it plainly the natural terminus of a series. The first we may call *the lingual*, and the other *the back-palatal*, or *the guttural*, *series*. The vowels of the lingual series are also allied by the general direction of the vocal current forward, while in *å*, *o* and *u* it is upward;—the position of the tongue for this effect may be observed to influence the lower jaw: tending to protrusion in at least *å*, *o* and *u*, and to retraction in at least *a* and *ä*. The plausible and commonly accepted scheme which regards these two series as determined by the less and less palatal opening from *a* to *i* and the less and less labial opening from *a* to *u*, fails to suit the facts as they present themselves under accurate observation.

Other lines of vowel transition diverge from the guttural series forwards toward *i*. Thus, the open *å*, *o* and *u* are so related to the open *ö* by proximity and the direction of the vocal current as to run readily into that. The middle and close *ö* are so related to the *ä* on similar grounds; the same *ö* vowels, as falling between a middle or close *o* and an *i* or an *e*, make the German *ö* of the *umlaut;* and, by a similar process, we have the *ö* vowels in the French *eu* from an original *e*+*u*. The connection is intimate between all degrees of the *u* with the *i*,—the tongue being so placed for the *u* that, by raising the fore-part, it readily comes into position for the *i*. A similar operation takes place between *å* and *i* in the Eng. *oi* diphthong, and between several different vowels (*ö*^{4} the proper one) and *i*, in the various ways of pronouncing the Eng. "long *i*." From the open vowels generally to the high position of the back tongue which forms the close or middle *u*, the transition is easy, at least in diphthongal combination, as will presently be exemplified. There is a special ground of transition between *i* and *u* in the similar positions of the soft-palate.

We are now prepared to consider the laws to which diphthongal combinations are subject; but I will first enumerate *the principal pure diphthongs that are possible*. They are:—

1. *a*+*i*:[14]—Eng. only in the word *ay*, or *aye*, or sometimes heard in Is*ai*ah, Sin*ai*, &c., and in the long *i*, wrongfully.
2. *å*+*i*:—*toi*l, b*oy*; North of England long *i*.

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[14] Instead of *i* non-labial as the final element, we may have, in each case, the labial *i* (Ger. *ü*, Fr. *u*). Dr. C. L. Merkel resolves the German diphthong *äu* (H*äuse*r, M*äu*se) and *eu* (F*eu*er, *Eu*le) into *a*+*ü*. His Physiologie der menschlichen Sprache (Leipsic, 1866, pp. 444) is an able and exhaustive treatise,—the author a thorough anatomist, and a careful and minutely exact as well as original investigator. Helmholtz and Brücke undergo the ordeal of sound and searching criticism at his hands.

3. $\ddot{o}^4+i^2$ (or, with the "glide,"[16] $\ddot{o}^4+\ddot{o}^3+i^2$):—long *i*, as high, pine, &c.,—the Scotch long *i* is $\ddot{o}^3+i^2$, or $\ddot{o}^2+i^2$.
4. $\ddot{a}+i$:—an affected pronunciation of long *i*.
5. $e+i$:—"long *a*" (*a*le) with the vanish.
6. $a+u$:—improperly in *ou*r, &c.
7. $\dot{a}+u$:—another wrong form of *ou*r, &c.
8. $o+u$:—"long *o*" (*o*ld) with the vanish.
9. $\ddot{o}^4+u^{2l}$ (with the glide, $\ddot{o}^4+u^3+u^{2l}$):—*ou*r, n*ow*, &c.;—the Scotch *ou*r, &c., are $\ddot{o}^{3l}+u^{2l}$.
10. $\ddot{a}^3$, or $\ddot{a}^4$, $+u$ (with glide, $\ddot{a}+\ddot{o}+u$):—Yankee *ou*r, n*ow*, &c.
11. e^4, or e^3, $+u$ (with $\ddot{o}$ glide):—ancient pronunciation of f*ew*, d*ew*, &c.; one form of Yankee n*ew*, r*u*de, sm*oo*th, &c.
12. i^4+u (with $\ddot{o}$ glide):—extreme form of Yankee n*ew*, r*u*de, sm*oo*th, d*o*, &c.
13. e^4+a:—Qu., A.-S. d*ea*f, c*ea*lf, &c.?
14. e^4+o:—Qu., A.-S. s*eo*fon, h*eo*fon, s*eo*lf, &c.?
15. u^4 or u^3+u^{1l}:—r*u*de, t*u*be, l*u*te, s*ui*t, n*ew*, d*ew*, &c.

The relative quantity of the initial and final elements is not alike in all these; but is usually greater in the initial. Where I have omitted to mark the degree, there is more or less latitude of variation.

The fundamental law of the pure diphthongs is, that the combination be such that the tongue can be, and is, kept continuously tense in passing from the initial to the final element, and the lips in like manner in the case of labials. Accordingly, the movement is more usually in a forward direction, and may be at the same time from open to close;—simply from open to close in the same group, though common enough, is not usually regarded as a diphthong, though I have included one such (No. 15) in the list above; indeed, combinations from two adjoining groups have not always been reckoned as diphthongs,—the usual English long *a* and long *o*, for example. A movement which requires a relaxation of the tongue or lips in passing from one element to the other will interrupt the continuity of the vowel sound, and necessitate either a hiatus, or the intervention of a *y* or *w* consonant, making in the latter case an *impure diphthong*. Thus, $\dot{a}+i$ (t*oi*l) is a forward movement, and gives continuous vowel sound; but the reverse, $i+\dot{a}$, almost necessarily introduces a *y* sound, heard as in *y*a*w*l ($i+y+\dot{a}$). So we have $i+y+a$ in *y*a*r*d, and in the Italian p*ia*no, f*ia*mma, &c.; $i+y+\ddot{o}$ in *you*ng, mill*io*n, bill*ia*rd, &c.; $i+y+u$ in *u*nion, m*u*te, &c. A rearward movement can give a pure diphthong only when the first element, if not both, is quite open; as in Nos. 9 to 14 of the foregoing table. A continuous movement from close to open on the lips always introduces a consonantal *w*, as in the French

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[16] A term used by phonetists, and denoting, strictly, the whole series intermediate, as the voice passes gradually from one position of the organs to another.

oui and r*oi*, the Italian bu*o*no, and English q*ua*rter, *war* ($u^{1l}+w+\mathring{a}^{1l}$), *we* ($u^{2l}+w+i^{1}$).[16]

A word here on the subject of *pitch as related to diphthongal sounds.* I have remarked upon the tendency to rise or fall in pitch as the tongue moves forward or backward, or else as it rises or falls, on our vowel-scale. There are certain noticeable phenomena on which this has a bearing:—*first*, the rising inflection of a single tone on each syllable in ordinary unimpassioned utterance,—pointed out by Dr. Rush,—and *second*, that, in the change from the radical to the vanish of the vowel,—so prevalent in English,—and in all the usual diphthongs in short, the tongue-movement is either forward or upward. These facts, supposing the above-mentioned tendency to be real, have a plain mutual connection. Notice also, in impassioned utterance, that the rising inflection brings out the vanishing element more distinctly and more naturally than does the falling: compare *No?* or *Nay?* interrogative, with the same word in the positive tone of authority or of confident assertion. It may be worthy of inquiry, whether in those languages which are less diphthongal the level tone more prevails.

Let us now attend to some of the applications of the system in *accounting for etymological and orthoëpical changes.* A process which figures largely in the history of vowel-change is the *condensation of a diphthong into a simple vowel intermediate* between the extremes of the compound. Thus, in French, we have *ai* run together into an *e* or *ä* sound,—as in *ai*mer, j'*ai*, f*ai*re, ch*aî*ne,—and *au* into an *o*,—as in ch*au*d, c*au*se, p*au*vre,—and *eu* into an *ö*,—as in p*eu*r, v*eu*ve, j*eu*ne. That these digraphs were once really diphthongal in utterance, is quite certain. (See Dietz, Gram., vol. i.) Such change is common in most languages. In describing the *ä* vowels (p. 184, note), I adverted to the originally diphthongal character of *ai* and *ay* in English, as in pr*ai*se, v*ai*n, d*ay*, s*ay*, &c. So was it also with the *au* and *aw*, which now take an *å* sound, as in f*au*lt, c*au*se, dr*aw*, l*aw*. In the Sanskrit, we find the *e* and *o* sounds existing only as developed from *ai* and *au*;—the *e* and *o* are always and everywhere, at least in the Indo-European languages, secondary and derivative elements, owing their origin to this or to some other process of transformation. By the *umlaut*, which fills so large a place in modern German, we have, from *o*, from *a*, and from *u*, in the root, an *ö*, an *e* or *ä*, and a *ü*, developed as intermediate by the influence of an *i* originally existing—though afterwards dropped or changed to *e*—in a succeeding syllable of inflection or derivation; as W*o*rt–W*ö*rter, H*a*nd–H*ä*nde, k*u*rtz–k*ü*rtzen; and as B*e*tt, *E*nde, from old b*a*di, *a*ndi;—relics of it in English are b*e*d, *e*nd, m*e*n, *s*ell (Goth. s*a*lyan), and other cases not a few;—and in the Old Norse, for instance, *a* takes umlaut from *u* in the next syllable. The umlaut is believed to have come up through an intervening stage of diphthongation,—b*a*di, for example, becoming first b*ai*di and then b*e*di or b*e*d: but whether so or not, reference to it here is pertinent.

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[16] Dr. Merkel, whose work comes into my hand as this article is passing through the press, approximates partially to the view presented above. He says, "The essence of the diphthong depends upon the convergence of the dilated vocal organs," and agrees with Brücke in ascribing the *w* and *y* to movement in the reverse direction, from close to open.

I maintain that such change is to be explained almost wholly by palato-lingual action. Of course it is so in the case of *e* or *ä* from *ai*. As for *au*, labial position would not determine its reduction to *o* or *å* rather than to a labial *ö*. In the *ü* from *ui* or *iu*, there is usually an admixture of a consonantal *y*, such as comes from a palato-lingual position between that for *u* and for *i*; and such may be presumed to have always appeared plainly in the first development of the *ü*. The labial quality is indeed imparted from the *u* to the *ü*. But, in Anglo-Saxon, the *y* which appears as umlaut of *u* was probably a non-labial; as the frequent interchange with *i* in writing would strongly indicate. The short *u* from which it arose was possibly non-labial, and the umlaut may have been distinguished from a quite open *u* or *i* by the consonantal *y* sound intermixed. Examples of this A.-S. *y* are b*y*rig, m*y*len, c*y*rice, c*y*mlic, c*y*ning, b*y*rden, b*y*sig, b*y*cgan, l*y*stan, l*y*tel, s*y*n, r*y*ne, (town, mill, church, comely, king, burden, busy, to buy, to desire, little, sin, running.)

The change above described comes rather as a natural result from the rapid utterance of the extremes in close succession than from an attempt to effect their simultaneous utterance. The developed product does not necessarily take the quality of either of the two extremes.

Change from a diphthong to one of its components, by dropping the other, is also not uncommon. Thus from A.-S. *ea*, we have sh*a*ll, sh*a*rp, h*a*rd, c*a*lf, &c., as well as d*ea*f, h*ea*d, r*e*d, &c.; from A.-S. *eo*, s*e*ven, h*e*aven, d*e*vil, &c.; and we have benef*i*t, par*i*sh, ven*i*son, compar*i*son, &c., from the old or the later French bienf*ai*t, par*oi*sse, ven*ei*son, compar*ai*son.

Another process of change is *from a simple vowel to a diphthong*,—commonly, if not always, effected by prefixing or annexing another simple vowel. Thus we have our "long *i*" ($\ddot{o}^4+i^2$) from a simple *i*,—in multitudes of words, of Anglo-Saxon, French, and Latin origin, such as w*i*ne, cr*i*me, t*i*me, dr*i*ve, wr*i*te, w*i*fe, w*i*ld, s*i*ght, *i*dle, *i*ron, &c.; and *ou* ($\ddot{o}^4+u^{2l}$) from *u*,—as h*ou*se, m*ou*se, h*ou*nd, from A.-S. h*u*s, m*u*s, h*u*nd, &c. In French, we find *oi* from *i*, as b*oi*re, d*oi*gt, f*oi*, m*oi*ns, n*oi*r, v*oi*e, v*oi*s, from Latin b*i*bere, d*i*gitus, f*i*des, m*i*nus, n*i*ger, v*i*a, v*i*deo, &c. The "guna" so common in Sanskrit is the strengthening of a vowel by prefixing an *a*. In French, we have a short *i* prefixed, in b*i*en, br*i*ef, d*i*eu, f*i*el, f*i*er, p*i*ed, t*i*ent, &c., and an *e* before an *o* that is transformed to *u*, in fe*u*, je*u*, ne*u*f, me*u*le, pe*u*ple, he*u*re, &c. (from f*o*cus, j*o*cus, n*o*vem, m*o*la, p*o*pulus, h*o*ra, &c.). In Italian, the *i* prefixed to *e* and *u* to *o*, form a marked feature: as f*i*ero, s*i*eda, b*u*ono, &c. Examples of the new element suffixed we have in our "long *a*" and "long *o*" with the vanish;—in French, we have *i* added to *a* of the Latin, in cla*i*r, *ai*mer, ma*i*n, &c.; *i* to *e* in O. Fr. me*i*, tre*i*s, le*i*, ve*i*le, &c. (now moi, trois, loi, voile, &c.), and in the modern fre*i*n, ple*i*n, ve*i*ne, &c.; *i* to *o* in vo*i*x, conno*î*tre, &c., and to *u* in su*i*s (s*u*m), &c.;—all of these digraphs having been once actual diphthongs. All such changes must accord with the laws of diphthongal combination, as before stated; to which may be added, that it is in converting a short vowel into a long one, and in giving greater quantity or weight to one already long, that the tendency to diphthongation is usually manifested.

From a diphthong thus produced, the original vowel may afterwards drop off, and through these two processes, a simple vowel may be replaced by another entirely different and perhaps quite remote on the scale. Thus, through Fr. br*i*ef, we have Eng. br*i*ef (with *e*, not *i*, silent), from Lat. br*e*vis; f*i*erce, through f*i*er, or f*i*ers, from f*e*rus; benef*i*t, through bienfa*i*t, from benef*a*ctum; g*o*vern, Fr. g*ou*verner, Lat. g*u*bernare; &c. So, in f*i*eld, f*i*end, p*i*erce, the superadded *i* is the only element now heard.

A change from one vowel to another may also take place by the roundabout process of *diphthongation and subsequent condensation* to an intermediate element.

A very important process of transformation is *when one vowel changes directly into another*, the two being nearly related by contiguity on the physiological scale, or by such modes of formation that the organs readily slide from one into the other. Sometimes it is a transition from one group to another, and sometimes a change of degree under the same group,—the routes from group to group being identical with the lines of easy transition above mentioned. Such transformation is often consequent on a change in quantity or in accentuation. Sometimes it is a strengthening and sometimes an attenuation; sometimes is promoted by influence of associated consonants, and sometimes by the assimilating force of a neighboring vowel. Very commonly it results from certain general tendencies prevailing in a language.

The changes of this sort in the *Italian as evolved from the Latin* are strikingly confirmatory of certain leading features of the scheme here set forth. The Italian exhibits a remarkable regularity in its development, having been little disturbed by outside influences,—and is thus, so far as it goes, peculiarly fitted for the establishment of principles of phonetic change. There are recognized in Italian an *i* vowel, and a "close" and an "open" *e*,—so called,—the close (*chiuso*) being like the *e* of our scheme, or nearer to the *é*, and the open (*aperto*) either precisely or nearly like the *ä*. So there is a *u*, and a "close" and an "open" *o*,—the close nearer to the *u*, and the open probably nearer the *ȧ*, than is the *o* in the scheme." Thus, *three* stand in order in what I called the lingual series, and *three* in the back-palatal, or guttural. Of each there is a long and a short; and these with the *a* are all the vowels of the language. As to their development from the Latin, the open *e*, says Dietz (Gram., i, 312 *et seq.*), arises (1) out of short *e*: as d*e*a, b*e*ne, br*e*ve, &c.; (2) out of *e* in position: as *e*cco, b*e*llo, t*e*mpo, &c.; (3) out of *œ*: as En*e*a, pr*e*da, &c.;—the close *e* arises (1) from short *i*: as bevere, cenere (from b*i*bere, c*i*nis), &c.; (2) from *i* in position: secco, esso (s*i*ccus, *i*pse), &c.; (3) from long *e*: as arena, cera, &c.;—again, the open *o* comes (1) from short *o*: as b*o*ve, *o*dio, *o*pera, &c.; (2) from *o* in position: as f*o*ssa, p*o*ndo, *o*rbo, &c.; (3) from *au* diphthong, also sometimes from long *o*;—the close *o*, (1) from short *u*: c*o*va, d*o*ge, s*o*pra (c*u*bare, d*u*cem, s*u*pra), &c.; (2) from *u* or *y* in position: d*o*lce, m*o*lto, *o*nda, t*o*rso, &c.; (3) from long *o*: on*o*re, glori*o*so, &c.;—the long *i* and long *u* of the Latin remain, and the *a* is unchanged.

Now, if we suppose the Latin *e* to have been either the *e* of our scheme or nearer to the *é*, and the Latin *o* to have been the *o* or nearer the *u*, we find *the same rules in each set of these changes.* We have (1) short *i* changed to so-called "close *e*"—*é* or *e* of the scheme, and short *e* to nearly if not precisely our *ä* vowel; and again, short *u* to "close *o*," and short *o* to nearly if not fully our *ȧ*;—and the resulting vowel is long by the Italian rules of quantity. That is, we have a long evolved from a short, together with a movement one station backward. All this accords with what was pointed out as a natural tendency physiologically considered: a short vowel is never a quite close one, and when lengthened tends mostly to greater closeness and often to transition backward. We have (2) *i*, in position, changed to "close *e*," and *e* to "open *e*;" and again *u* to "close *o*," and *o* to "open *o*." Here also we have the original vowel short, though in a syllable long by position; and though the vowel does not become technically long, the change is not improbably connected with a greater weight and predominance of the vowel sounds in modern Italian as compared with the Latin,—as appears in the frequent use of *ie* and *uo* in place of simpler vowels; or, the change may be ascribed to a general tendency to transition backward

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[‖] It may seem that one or two more vowel-places should be marked on the back-palatal part of our scale, since the ear can distinguish the sounds—though indeed only for the close or close and middle degrees: perhaps so many ought to be noted,—certainly if demanded by the exigencies of any single language.

prevailing in Italian,—as does the reverse in English. We have (3) an original long vowel or diphthong, with little or no change except the condensation of the diphthong,—just as ought to be expected in connection with the actual changes as above stated. Similar laws and tendencies have had partial sway in the other branches of the Latinic family.

Every language under the sun will show examples, in abundance, of changes, more or less regular, by direct transition from one vowel group into another,—whether we study it in its etymological history, its dialectic variations, or the mutations of orthoëpical fashion. *The changes in vowel-pronunciation which the English has undergone*, and for the most part within three hundred and fifty years, are many, though not all, of this description. It should be received as an incontrovertible fact, that the vowels in English had once substantially the Latin and Italian sounds;—and this they had, indeed, for the most part, even to the early part of the sixteenth century. The long *u* had not then become diphthongal, as it now is. The long *i* was diphthongal; but probably as e^2+i^1. The long *e* and *ee* had then, a very few cases excepted, the proper *e* sound, heard still from the Irish in "*swate*," "*indade*," for *sweet*, *indeed*, and the like. Two hundred years ago, the *ee* had obtained its present pronunciation as in *ee*l, but the long *e* single, and the *ea* and *ei* still retained substantially an *e* sound. The change of the long *a* has already been spoken of as having occurred since that time. The digraph *au* had its present pronunciation at the earliest of the two periods just mentioned; it was between these periods that the *oo* came to its present sound (f*oo*t, f*oo*d) from that of long *o*. Two hundred years ago, the diphthongs *ou* and *oi* (*ou*r, *oi*l, &c.) had the initial element not precisely as at present; the initial of *ew*, in f*ew*, &c., was more usually an *e*, instead of, as now, an *i* sound; while the first in *ai*, *ay*, was a proper Latin or Italian *a*, or wavered between this and an *ä*.[18]

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[18] For a full exposition of one branch of this topic, see the article, *Shakespearean Pronunciation*, in the North American Review for April, 1864. In respect to the long *a* as in Shakespeare's time, the view there taken is not quite correct. A careful examination of Wallis and Wilkins leaves no doubt that they regarded it as identical with the Italian *a*. Dr. Wallis (Gram. Ling. Ang., 6th ed., 1765, p. 8) says of the English *a*, long and short: "Cambro-Brittani hoc sono solent suum *a* pronunciare; Italique suum;" and again (p. 55): "seu ut *a* Italorum." He describes it as the slender *a*, "*a* exile," in distinction from the broad *a*, "*â* pingue," used in the German and the French of that period, and heard in the English *all*, *hall*, *haul*, &c. Again, he describes the English *e* (pp. 9, 56) as like the *e* of the French, Spanish and Italians,—which identifies it, when long, substantially with our present "long *a*"; and he describes it further as intermediate between *a* and the *ee* in f*ee*t, or the *i* of the continent. His English *a* must clearly have had either a proper *a* or an *ä* sound;—also, the statement, in the same article, as to the *e* of that period, is seen to need correction. Bishop Wilkins, in his phonetic transcript of the Lord's Prayer and the Creed (Real Character, p. 378), employs one character for the *a* in f*a*ther, *a*rt, h*a*llowed, n*a*me, *a*s, d*a*y, d*a*ily, tresp*a*sses, tempt*a*tion, *a*nd, *A*men, m*a*ker, M*a*ry, Pil*a*te, w*a*s, *a*g*a*in, *a*t, h*a*nd, c*a*tholic, everl*a*sting, and for *o* in b*o*dy. He uses an-

The point we have in hand,—that of direct transition from one vowel-group to another,—will be best illustrated *by taking into view, etymologically, the ancestral and allied tongues along with the English*, and adverting also to existing dialectic variations,—with this proviso, however, that the precise process of change cannot always be determined as direct rather than indirect and circuitous.

In the *a* group, we find a widely prevalent tendency to move in two separate directions: upon the lingual line to *ä* and still on to *e*, *é* and *i*, and upon the back-palatal line to *å* and yet beyond to *o* and *u*. Thus, from Sanskrit pad-as, we have, on the one hand, the Latin ped-is, and on the other, the Greek ποδ-ός, A.-S. fôt, Eng. foot; again, Sans. dant-as, Lat. dent-is, Gr. ὀδόντ-ος, A.-S. tôdh, Eng. tooth; Sans. ashtan, A.-S. eahta, Eng. eight, Lat. octo, Gr. ὀκτώ; Sans. naman, A.-S. nama, Eng. name (*e*), Lat. nomen, Gr. ὄνομα; Sans. jânu, Lat. genu, A.-S. kneo, Eng. knee, Gr. γόνυ; Sans. matri, Lat. mater, Gr. μήτηρ, A.-S. modor, Eng. mother (ŏ), Ger. mutter; Sans. mas, Gr. μήνη, A.-S. mona, Eng. moon; and so on, indefinitely. So we have A.-S. bân, stân, hâm, &c., Eng. bone, stone, home, &c., Scottish bane (*e*), stane, hame, &c. The tendency of *a* to *å* is widely observable. The High German *a* is *å* in Low German dialects. The *a* in French vibrated into *å* in the seventh century, and so continued nearly through the eighteenth, when it swung back to *a*.[18] *Hand*, *stand*, *land*, *sand*, and the like, in the early English, we find most frequently spelled *honde*, *stonde*, *londe*, &c., even as late as Tyndale; and either this pronunciation was not universal or there was afterwards a return to the *a* sound. The Gothic had no long *a*, but a long *o* corresponding to the long *a* of the Sanskrit and employed whenever the law of etymological change would lengthen the short *a*. The sound was, undoubtedly, at first an *å*, but had come so near to *o* before the language was reduced to writing by Ulfilas, as to induce him to employ that letter. The long *o* in English comes but rarely from Anglo-Saxon long *o*, and frequently from A.-S. long *a*; we may, therefore, presume that it had not at first a proper long *o* sound, but

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other for the *a* in almighty, and the *o* in Lord, of, for, from, &c.; and employs *e* for conceived, dead, Jesus, Amen, heaven, earth, &c.,—that is, for a sound which, when long, did not differ greatly, if at all, from the "long *a*" of our time. Reference to these two important authorities would have corrected the like error, or what I cannot but regard as such, on the part of Mr. Marsh, in his Lectures on the English Language, first series, Lect. xxii. I have not overlooked the noteworthy, but inconclusive, "Memoranda" of Mr. R. G. White, appended to vol. xii of his edition of Shakespeare.

For the earlier period referred to above, I have depended mainly upon Palsgrave, L'Eclaircissement de la Langue Française, the reprint by Genin, Paris, 1852 (1530 the original); for the other and later, my statements accord with those of Wilkins and Wallis. See also Atlantic Monthly, vol. iii, pp. 255, 6 (in the article on White's Shakespeare).

[19] See the article in the North American Review, referred to in the preceding note.

nearer to *å*; and the presumption is confirmed by the fact that the A.-S. long *o* became the English *oo*, and that this *oo* expressed the long *o* sound till at least the sixteenth century. The transition on this line is naturally from the long *a*; while the short *a* would move directly only on the other line, through *ä*: thus we have in Anglo-Saxon a short *ä* as the more frequent correspondent of Gothic short *a*,—as A.-S. d*ä*g, m*ä*y, *ä*t, th*ä*t, b*ä*d, g*ä*rs, g*ä*f, &c., for Gothic d*a*gs, m*a*g, *a*t, th*a*ta, b*a*th, gr*a*s, g*a*f, &c. (Eng. day, may, at, that, bade, grass, gave, &c.).[20]

Attending now to *the lingual line*, we have no need to add to the instances already mentioned—our English "long *a*" among the rest—of gradual change from *a* through *ä* to *e*. From *e* on to *i*, examples are not less numerous. Thus, the *i* sound as expressed by *ee* in English, in *ee*l, d*ee*m, &c., has arisen gradually by transition from the *e* sound, which the *ee* had in fact in early English,—and would have as replacing, for the most part, the A.-S. long *e*, while otherwise, with one or two exceptions, it is from A.-S. *ae*, *ea*, or *eo*. The same sound expressed by *ea*, *ei*, *ie*, as in h*ea*r, rec*ei*ve, bel*ie*ve, &c., whether from the French or the Anglo-Saxon, is almost always descended from an *e* sound. Many words are easily traced all the way from *a* to *i*: as str*ee*t, from the Latin str*a*ta, through the A.-S. str*æ*te and str*e*te; *E*ngland, with now an *i* sound, from *A*ngleland, through *Æ*ngleland and *E*ngland; p*ee*r, from Lat. p*a*r, O. Fr. p*ee*r, p*e*r; and agr*ee*, from Lat. ad gr*a*tum, Fr. agr*ee*r. In the Irish brogue, we notice the open *i* in place of open *e*: as r*i*nt, t*i*nant, g*i*ntlem*i*n, m*i*mber of parl*i*m*i*nt, &c. The tendency of the English has always been in a forward direction on this line, and on the others as well; yet sometimes we have the reverse movement, as in gr*a*ss, f*a*ther, from A.-S. gr*ä*s, f*ä*der, &c., and as our Southerners, some of them, both the white and the black, say *shore* for *sure*, *pore* for *poor*, and *star*, *har*, for *stair*, *hair*. The Yankee dialect goes beyond the common English in transition forward on the lingual line: as in g*i*t, y*i*s, *e*s, k*e*tch, for g*e*t, y*e*s, *a*s, c*a*tch, &c.; the same general tendency which leads to this inclines also to substitute the open non-labials, in the *u*, *o* and *å* groups, for the close labials, as requiring a less wide opening of the jaws, and in general less effort of the organs.

The French, as from the Latin, affords numerous examples of direct transition on the lingual line, in both a forward and a backward direction: as ch*e*r, s*e*l, t*e*l, from c*a*rus, s*a*l, t*a*lis; s*i*x, l*i*t, c*i*re, n*ie*r, *aimé*, from s*e*x, l*e*ctus, c*e*ra, n*e*g*a*re, *a*m*a*tus; m*oi*, cr*oi*s, av*oi*r, from m*e*, cr*e*do, hab*e*re; cons*e*il, just*e*sse, poss*è*de, v*e*rre, from cons*i*lium, just*i*tia, poss*i*deo, v*i*trum, &c.

On the back-palatal line, we find in English the *u* sound, in words of Anglo-Saxon origin, most frequently from an A.-S. long *o*, as f*oo*t, g*oo*d, b*o*som, c*oo*l, st*oo*l, t*oo*th, d*o*, &c.; and as the A.-S. long *o* corresponds to the Gothic long *o*, and this to the Sanskrit long *a*, we have a regular precession from *a* to *u*. Examples in abundance to the same effect might be adduced from other languages.

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[20] See the Grammars of Fiedler and of Mätzner for a full detail of letter-changes in the derivation of the English from the parent tongues.

On what we might call the *upper-palatal line*, from *u* to *i*, with only a consonantal *y* intervening, a familiar example of direct transition is the French *u* (i^l) from a Latin *u*,—most frequently from a long, though sometimes from a short *u*. The same change is found to have occurred in the Italian of Lombardy, and one similar took place at some period in the Greek; the Polish *y*, which is a non-labial, but corresponds etymologically to Sanskrit *u*, probably arose in a similar manner. The Scotch g*ui*d, or g*u*de, m*ui*r, s*u*ne, s*u*ld, b*ui*k, (for g*oo*d, m*oo*r, s*oo*n, sh*ou*ld, b*oo*k,) &c., are noticeable in this connection. The vowels which spring from the *u* on this line, whether by direct transition or by the umlaut are somewhat diverse in themselves; but all admit of further precession or attenuation into an *i* non-labial and such as to betray no trace of origin from *u*. This has come to pass in dialects of Lombardian, French, Modern Greek, German, and other languages (see Dietz: Gram. I, 415); in English we have numerous cases of *i* from A.-S. *y* as umlaut of *u*,—as k*i*ng, s*i*n, k*i*n, br*i*dge, l*i*ttle, &c.

As for the transitional connection of the ö group with the others,—we may observe in the German ö and French *eu* somewhat of a proclivity to slide into a close ä in pronunciation. In our v*i*rtue, m*e*rcy, *ea*rth, &c. the vowels here followed by *r* have fallen back into an ö sound. The open ö has a near relation to the open vowels of several groups,—as its place on the physiological map would render probable. Transition into it is easy especially from *u*, *o* and *å*; and has taken place in s*u*n, b*u*t, *u*p, m*u*tton, in the vulgar t*u*ck for t*oo*k, s*u*t for s*oo*t, &c., and in s*o*n, d*o*ne, m*o*nkey, n*o*thing, m*o*ther, br*o*ther, &c. We know not whether the m*i*ther, br*i*ther of the Scotch are examples of *i* from ö, or from *o* by way of *u* in a form like the Ger. m*u*tter, br*u*der; but the latter would seem the more natural. From *a*, *e* and *i* to ö open, instances are numerous in unaccented syllables: as, liv*e*r, ov*e*r, robb*e*r, pill*a*r, doll*a*r, elix*i*r, nad*i*r, probl*e*m, pr*e*dict, tyr*a*nt, wom*a*n, Cub*a*, Missour*i*, def*i*n*i*te, d*i*gest;—this before *r* is correct, but not approved in most other cases.

The *relations of certain vowels to certain of the consonants*, are well explained upon our scheme. Thus, the positions of the tongue for the open ö and for the usual English *r*,[21] requiring but a slight change to pass from one to the other, favor the use of this vowel before the *r*,—as in cases just cited. Again, it often occurs that *l* gives way for a *u* in its place: as Fr. a*u*tre from a*l*ter, sa*u*f—sa*l*vus, fa*u*x—fa*l*sus, bea*u*—be*ll*us, cheva*u*x for cheva*l*s, d*u*—de *l*e, a*u*—à *l*e, &c.; North-English a*w*most, a*w*d, for a*l*most, o*l*d; Scottish ha*u*d, ha*u*f, a*w*mous, sho*u*ther, &c. for ho*l*d, ha*l*f, a*l*ms, shou*l*der, &c. In the English *would, walk, talk*, &c. the *l* probably first gave way to a *u* sound which with the vowel preceding coalesced into one intermediate; and the *l* still heard in *altar*, *ball*, &c. probably had influence in the change from an *a* to an *å* sound. It sometimes occurs that *u* is prefixed, the *l* being retained, of which *would*, from *wolde*, must have been originally an example. Now, as *l* is not a labial but a lingual consonant, this relation cannot be accounted for at all, if we regard *u* as simply a labial, but is easily understood if we attend to the palato-lingual action, in the vowel as in the consonant. In forming the *l*, while the tip of the tongue touches the palate, the posterior portion also is raised and placed nearly as for the vowel *u*. Hence, when *l* is weakened by relaxation of the tip of the tongue, it naturally falls into *u* in certain cases,—and this occurs especially when coming after a vowel and before a consonant. But, in certain other cases, *l* passes into *i*: as Italian p*i*ano from p*l*anus, f*i*amma—f*l*amma, ch*i*aro—c*l*arus, p*i*acere—p*l*acere, ch*i*amare—c*l*amare, b*i*anco—Fr. b*l*anc—Eng. b*l*ank. Here *l* follows a consonant and precedes a vowel,—and the *i*, whose place reaches nearly to the tip of the tongue, results naturally from the weakened *l* in this case.

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[21] On the difference between the English and the continental *r*, as well as on other nice points in comparative phonology, see the very carefully and well prepared Introduction to the *Dictionary of the Noted Names of Fiction*, by William A. Wheeler, (Boston, 1865.)

The frequent interchange between the vowel *u* and the consonants *w* and *v* is of course to be explained by reference to labial action.

In the case of the palatal, or gutturo-palatal, mutes, *k*, *g*, *ch*, (tenuis, media, asper,) with the related sibilants included, the variation induced by association with different vowels is altogether accordant with our scheme. Thus, in German, the difference of the *ch* in *nach* or *noch* and in *nicht* or *mich*, is too plain to be overlooked; and with the vowels throughout, this consonant articulation is more or less deep—near the throat, that is—in the order of our groups, *a*, *å*, *o*, *u*, *ö*, *ä*, *e*, *é*, *i*. So with *g* (hard) in English, the sound is really different, and certainly produced at a different place, in g*a*pe, g*o*ne, g*o*, g*oo*se, g*i*rl, g*a*rish, g*a*y, g*ea*r. In *Egypt*, we find it difficult, if we try, to give the hard sound at all to the *g*, coming thus between two vowels of the extreme anterior group. In words like *kind, card, guile, guard*, &c. a precession and partial softening of the consonant necessarily changes the succeeding vowel, in accordance with a quite prevalent style (Princ. of Pron. § 72, Note), either by interposing a vowel from a forward group, or, as may be done with k*i*nd, g*u*ile, &c. by a mere precession of the initial of the dipththong: and, *v. v.* the change of the vowel would necessitate that of the consonant. Especially noticeable is a frequent change of Anglo-Saxon *g* into *u* or *w* and into *i* or *y* English, as determined by the antecedent vowel. Thus come m*aw*, dr*aw*, s*aw*, *ow*n, b*ow*, &c. from m*ag*a, dr*ag*an, s*ag*e, *äg*en, b*og*a; and m*ai*n, m*ai*den, w*ai*n, s*ai*l, r*ai*n, l*ai*d, m*ay*, d*ay*, w*ay*, &c. from m*äg*en, m*äg*den, w*äg*en, s*eg*el, r*eg*en, l*eg*ede, m*äg*, d*äg*, w*eg*, &c. We have also m*au*dlin from M*ag*dalen. To explain fully the consonant gradation, it must be added that, while *k* and hard *g* always involve contact guttural, that is, of back-tongue with back-palate,[22] the point of contact may be higher or lower; it will be higher as the vowels associated advance forward on the scale; and, at the same time, the borders of the tongue will be applied to the palate in the precise place as for the vowel. A position will thus be reached which, omitting then the guttural contact, will give *y* for *g*, and German middle *ch* aspirate for *k*; or, replacing the guttural by forward contact, will give *g* soft, as *g*em, for *g* hard, and *ch* soft, as *ch*ill, *c* Italian, for *k* or hard *c*,—and, from these, transition is easy to Fr. *j*, or *z* in azure, to *sh*, or Fr. *ch*, and to *s*, or *c* in city. The vowel relations of the hard and soft, among these consonants, are to some extent familiar to all.

Finally, this view of the mode of formation of the vowels *gives the clue to the fundamental difference between vowel and consonant.* Of course the difference is plain enough in the case of the mutes *p*, *t*, *k*, and *b*, *d*, *g*, in which the sound is shut off from issuing through the mouth, as it is also in the nasals, *m*, *n*, *ng*. Again, in *f*, *th* (in *thin*), *sh*, *s*, &c. we have simply aspiration, or breath-sound, made at the articulating station, and in *v*, *th* (in *this*), *z*, &c. we have have the same mixed, or attended, with tone from the larynx; these all differ thus from the vowels, which consist of pure tone variously articulated. But we have *w*, *y*, *r*, *l*, which are, or at least may be, articulated with pure tone unmixed with breath sound. How, then, do these differ from the vowels?

Some phonologists hold that there is here no essential difference. There is a theory, strenuously advocated by Prof. W. D. Whitney, of Yale College, and by him first distinctly propounded, so far as I know, which regards the degree of open or close as the real and only ground of distinction between vowel and consonant,—what characterizes the vowels as such being their openness, and the consonants, their closeness, while a neutral border-land lies between, neither decidedly one nor the other."[33]

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[22] For *k* and *g* hard, the closure is always upon the soft-palate; made by the middle tongue and hard-palate it can give only an imperfect *t* and *d*,—Dr. Brücke's view to the contrary notwithstanding, for which see Prof. Packard's article, *Brücke's Physiology of Speech*, in the Bibliotheca Sacra for April, 1866, and which, it may be added, is fully disproved by the exact observations of Dr. Merkel,—though indeed the nature of the case, rightly considered, would render other disproof well-nigh superfluous.

Against this view,—which the well-merited reputation of Prof. Whitney as a phonologist entitles to very high respect,—it is to be observed that, in recognizing only a difference of degree—a difference of the same sort as exists among the vowels themselves—it fails to account for the marked difference in function between the two classes of elements. So that, if the contrast in degree of open and close be admitted as an actual fact, it is still not the material fact in the case. But the fact so assumed is far from being evident. Certainly, the *ch* aspirate is, in some cases at least, more open than some of the vowels; the usual English *r*, and still more the *r* made by trilling the uvula, is less close than the vowel i^1; and the consonant *w* and the vowel u^{1l} may be so uttered—in the word *woo*, for example,—that the latter shall be closer, labially, than the former. As respects both *w* and *y*, more will be said presently on this point.

Resuming our inquiry, it is to be observed that *w* requires no special palato-lingual position, any more than does *v*; as let be tried on the word *way*. It is true that a lip-modification like what belongs to *w* may be taken by the vowel u^{1l}. This will make what may be called either an impure vowel or an impure consonant, and may fulfil the function of either a vowel or a consonant: of a consonant in a word like *we*, or *woe*, and of a vowel in one like *food*, or *good*, when uttered, of course, in the way supposed. A similar mixture of vowel i^1 with consonant *y* is not unusual, and some vowels may take a trill of the uvula as an accompaniment. But vowel and consonant quality are nevertheless distinct; and *w* as a consonant is not at all a palato-lingual articulation.

There remain *r*, *l*, and *y*: they are palato-lingual articulations; which are, or it is here admitted may be, uttered with pure tone; which also allow of indefinite prolongation;—agreeing in these respects with the vowels. But, to each of these, as to every consonant, there is wanting what we have had occasion to notice as a feature pertaining universally to the vowels,—the firmly-walled tube between tongue and palate, made by tension of the lingual muscles, with those of the soft-palate besides, so that the sound from the larynx is reverberated and reacted on as it passes through.[34] In the consonants just mentioned, there is merely presented a partial obstruction, of a yielding nature, over which the vocal current breaks, or by which it rubs, producing a murmur, burr, or trill, only, instead of a reverberation and ringing out of the sound.

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[33] See the two articles, by Prof. Whitney, *On Lepsius's Standard Alphabet*, in the Journal of the American Oriental Society, vols. vii and viii.

[34] The fact that each several vowel not only requires a special palato-lingual passage, but that this passage is a proper tube,—walled in all round by closure on each side,—is a point to which particular attention is desired, as it seems to have been hitherto unnoticed. The side teeth, which may be considered as forming a virtual extension of the arch of the hard-palate, are, it should be observed, a not unimportant auxiliary, as a sort of buttress against which the tongue may brace itself in position for the open anterior vowels.

Turning again now to the *w*, we find here a contrast of tension and non-tension of the lips as a further distinction between this consonant and the vowel u^{1l}. This may be made visible and palpable in the word *woo*, which presents the two elements quite open as labials to external observation.

In explaining the theory of the diphthong, there was occasion to notice the development of *w* between two vowels whose succession involves transition from a close to an open position of the lips,—as in the Fr. *oui* and the Eng. q*ua*rter,—and of *y* in like manner in the transition from a forward to a backward (or what some would call a close and an open) palato-lingual articulation,—as in mill*io*n, bill*ia*rd. Now, such transition requires a relaxation of the tension which characterizes vowel-articulation; and either a hiatus or a consonant must intervene. The simple relaxation of the articulating organs, with continuance of the tone, gives us *w* in one case and *y* in the other. From this we may conclude, first, negatively, that these consonants are not more close than the vowels from which they are developed by transition to a more open position; second, positively, that this relaxation, or non-tension, is what essentially distinguishes these consonants from the vowels.

It is admitted by Prof. Whitney (Jour. of Am. Orient. Soc., viii, 361–2) that *w* and *y*, as consonants, are sometimes more open than some of the vowels,—an admission which would seem difficult to reconcile with his theory. He says of them, also, "They are nothing but *i* and *u* themselves, deprived of the quantity and stress which belong to a full vowel utterance" and thus "assuming a consonantal value." But is it not plain that the ear discerns something positive in *y* and *w* which is not heard in *i* and *u*? Have we not, moreover, in the "vanish" in f*ai*l, *ai*sle, t*oi*l, s*ou*l, f*ou*l, an *i* or a *u*, deprived, to the utmost, of quantity and stress, yet making no approach to a *y* or a *w*? Quantity and stress are, neither of them, essential to a vowel; and a *y* or a *w* admits in fact of indefinite extension in quantity. The ready capability of the vowels for quantity and stress, we may well regard as due to the tension of the articulating organs rather than to their relative openness; and it does not, in the vowels, vary as the degree of their openness varies, for we have it admitted (ibid. p. 355) that the short vowels are more open than the long.

The *characteristic feature of the vowel* depends, then, upon *having a palato-lingual tube formed in the manner described for the reverberation of the sound from the larynx*, and consists in the actual production and modification of the voice in that manner. When we add that, for the labials, the voice is further reverberated in a passage formed for such effect by the action of lips and cheeks,[*] we have stated all that pertains to the physiological definition of a vowel. But our view will be incomplete unless we see how it is that the vowel is thus fitted for its peculiar function in speech. It is possible to form a word or a syllable,—that is, a distinct, well-defined, vocal utterance is thus possible,—by a combination of mute and fluid consonants, or with a fluid consonant alone. *Pl*, *bl*, *cr*, *kn*, *prb*, *tlb*, *rl*, *sl*, *ll*, *rrr*, &c. are utterable without the help of a vowel;—but yet are never employed as words, though sometimes as dependent syllables. Why is it that a vowel, rather than a fluid consonant, is made a constituent part of every word? In the first place, the vowel gives to the voice a fullness, volume and force, a resonant quality, such as a fluid consonant can not. The voice finds, moreover, in the reaction it receives from the firmly braced yet unobstructive organs, a support on which it can lean, as it were, while still going out with free vent and full power. Upon the vowel, therefore, the main stress of the word is made to fall; and upon it the consonant elements hang in a relation of dependence, uniting with it easily and naturally as they do, whether prefixed or annexed, or both. Again, the vowel serves, by the stress and force it naturally takes, to mark off each separate word in connected speech,—that is, to give to each its unity and individuality as a word,—as must have been plainly the case in the original monosyllabic form. The superadded consonants render possible that variety in words which the ends of language require, and complete that capacity of fit expressiveness, in the word as related to the idea, which must once have been conspicuous, and of which all the traces are not yet absolutely obliterated. Thus it is that vowel and consonant concur to the bringing forth of the word. A few such elements, which take their character severally from processes of articulation, make up, by their varieties of combination, the whole of the outward form, or body, of that divinely ordained product of human instinct and intellect which we call speech or language, and which, in its various relations, presents one of the most interesting, and certainly not least important, subjects of scientific study.

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[*] The fact is, that the want of resisting power in the lips, as against the vocal current, disqualifies them from acting alone in the articulation of a vowel,—limits them to the subordinate and secondarily modifying agency as above and before described. That the palato-lingual is really the primary agency in the labial vowels, may be readily proved by pronouncing, as can be done easily and with perfect distinctness, an *ö*, an *u*, an *o*, and an *â*, with one and the same lip-modification for each and all.

ART. XLIII.—*Conclusive proofs of the animality of the ciliate Sponges, and of their affinities with the* Infusoria Flagellata; by H. JAMES-CLARK, A.B., B.S.

BEFORE I proceed to the main point in question in this article, I wish to say a word in regard to the group of animals, viz: the *Protozoa*, of which I am fully convinced the *Spongiæ Ciliatæ* are a part.

From the time when Ehrenberg published his great work, the "*Infusionsthierchen,*" to the period of the issue of the "*Études sur les Infusoires,*" of Claparède and Lachman, there has been a steadily growing belief that a large part of that mass of animalcules which Ehrenberg denominated *Infusoria* forms a distinct grand division, equally as decided in character as any of the four great groups which are now generally accepted. Still it is a little curious that, although Cuvier had long ago pointed out the *plan* or type upon which his four *embranchements* were constructed, later investigators have not attempted to elucidate the *typical idea* which lies at the basis of the Protozoan organization. Claparède and Lachman have approximated nearest to such an attempt in their division of a part of the group into dexiotropic and læotropic sections, but nothing is said, even there, of a plan which runs through the whole grand division. Surely they had seen enough of material—at least of the higher divisions of the group—to sustain them in pronouncing upon the typical relation of the Infusorian organization; but it may be that the apparent paucity of characters among the lower members of this grand division misled them into an apprehension that there was no definite taxonomical relationship of the organs. That they recognized the latter as members of the same group with the former no one will deny, but it must be conceded that the affiliation was observed to be only one arising from similarity of organization and habit, and not from any community of *plan* in the disposition of the organs.

It is now over two years since I demonstrated—in a course of lectures, delivered in February and March, 1864, at the Lowell Institute in Boston—that the arrangement of the organization of the *Protozoa* is based upon a spiral, or rather a helix: more recently those lectures have been published[1] and the *type* of the organization of the *Protozoa*, as well as that of each of the other four grand divisions of animals, made as clear, by illustrations, as the limits of the volume seemed to allow. In order therefore that I may not appear to claim for the Sponges merely a new position in the universe of obscurities, I shall take the liberty of drawing the reader's particular attention to the arguments which I have adduced—in the volume above mentioned—to prove the unity of plan in the organization of the *Protozoa*, and its dissimilarity from any other which dominates among the four remaining grand divisions.

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[1] H. James-Clark: "*Mind in Nature.*" D. Appleton & Co., New York, 1865.

This much being premised, I proceed now to give a sketch of the peculiarities of some of the genera of *Infusoria Flagellata* with which I think the Sponges are most intimately associated. Several of these genera are new to science, and moreover of the most remarkable forms. I regret that words alone cannot, at this time, render their peculiarities as evident as I hope the illustrations will, in my forthcoming paper, in the Memoirs of the Boston Society of Natural History.

I must ask the reader in the first place to go back with me almost to the Ultima Thule of animal simplicity, and revise the organization of the hitherto too lowly estimated *Monas*, in order to lay the foundation for the group which embraces in its limits so gigantic a family as the *Spongiæ Ciliatæ*. I do not think any one will be prepared to fully appreciate such a remarkable definiteness and system in the arrangement of the organization of *Monas* as I have discovered among the various forms which constitute that genus.

Hitherto a *Monas* has been looked upon as a mere shapeless molecule, with a vibrating *cilium* of some sort or other, attached to its surface at an indefinite point. As I understand the relation of parts, now, the *motory cilium* or *flagellum* is perhaps the most remarkable feature of the whole animal, not only in a physiological aspect, but also in its topographical relationship. Let me illustrate this by a description of the body and appendages of *Monas termo* Ehr.

The body of that species has the form of a wide, compressed heart, with two distinct summits. The broad flattened sides lie opposite to each other, and parallel with the plane which passes through the two summits, and which forms the prolongation of the greater transverse diameter of the body. Between these summits is an aperture which constitutes the mouth. One of the summits is prolonged into a broad, conical, beak-like body, and assists the mouth in the prehension of food. It is therefore a true *lip*. The *flagellum*, however, is the real prehensory organ, although it, at the same time, performs the office of a propelling agent, when the body is detached from its pedicel and moves about in a free state. This organ has the form of a scarcely tapering bristle, which is attached close to the edge of the mouth, on that side of it which is opposite to the lip, and rises with a decided, well-defined curve whose plane is coincident with the plane which runs through the two summits, and forms, as I have just mentioned, the plane of the greater transverse diameter of the body. This remarkable feature is scarcely to be recognized during the free state of the animal; but when the latter is moored by its posterior end to its pedicel, the phenomenon in question is very marked and conspicuous. For most of the time the *flagellum* sustains itself in this rigid, arcuate position, and is always curved away from the lip; but its terminal end keeps up an almost incessant spasmodic incurvation toward the mouth, to all appearances for the purpose of throwing, or jerking, particles of food in that direction. When an acceptable morsel is met with, both the lip and the *flagellum* combine to press it into the open jaws of the animal; and when that is accomplished, the two organs immediately return to their former positions.

Scarcely less noticeable is the so-called *contractile vesicle*—the analogue of the heart of the higher animals. In a view of the body, so placed that the lip is next the eye, and the *flagellum* consequently curving away from the observer, we have the two broad sides on the right and left, and the plane of the greater transverse diameter coincident with the line of vision. The body then seems, at first sight, to have a symmetrical aspect, such as is not observable from any other point of view; and such it might be made to appear if I should belittle the importance of one single organ, by simply mentioning its existence, and omitting to lay down its exact topographical relationship. I refer to the *contractile vesicle*. During the systole of this organ it is so inconspicuous that it would easily escape even the most careful observation; but during the transition to the expanded state, and at the full diastole, its prominence, from the point of view just mentioned, is so great as to rival the *flagellum* in attraction. It may then be seen as a comparatively large, rounded, transparent, vesicular body, which stands out in strong profile, just in front of the middle, and close to the surface of the left side of the body. At full diastole it even forces the overlying region outwardly into a quite prominent papilla. In reference to the other organs and parts of the body, it stands, therefore, altogether in an asymmetrical relation; and from whatever point of view it—or any of the organs—may be observed, the organization as a whole evidently rests upon an oblique basis. The *bilaterality* of the type is sufficiently clear, but the topographical relationship of the organs is incompatible with *bisymmetry;* for right and left are twisted upon each other.

So much for *Monas*. As for the objection which has been raised against the estimate that has been put upon the monad-like Infusoria, because they have not been proved to be adult forms, it seems to me that the *onus* of proof lies on the other side, viz: to show that they are not adult. I think moreover that I am fully warranted in assuming that a *Monas* which possesses such an organization as I have described, and is attached to a stem, is an adult; and more especially so since, among many hundreds which I have observed from time to time, I have never seen any trace of a transition to a higher form. That such simple organizations can exist without rising to a more complicated state, during a whole lifetime, I am furthermore sustained in believing by the discovery of some new generic forms, which, although scarcely, if at all, more highly organized than *Monas*, have in addition such characters as would seem to stand in the way of a transition to a more elevated grade of existence. For instance, the presence of a *calyx* about the body of an infusorian, into which it can retreat, is an indication of a fixity of condition which corresponds to the adult state. Thus I found one of the new genera which I just alluded to.

Bicosœca, as it is called, may be described in general terms as a stemless *Monas* which is attached to the bottom of a calyx, by a highly muscular, retractile cord. All the organs have the same remarkable definiteness of relationship and peculiarity of form that *Monas* possesses; and in addition there is the muscular cord which, with oft-repeated jerks retracts the body to the very bottom of the calycine envelope. There are two singularly diverse species of this genus; one marine and the other lacustrine.

The most interesting infusorian of this group of new forms is the one which I have called *Codosiga*. This links the Sponges to the flagellate Infusoria. Its greatest peculiarity consists in the possession of a highly flexible, extensible and retractile, membranous collar, or hollow cylinder, which projects from the anterior end of the body. The cylinder is slightly flaring, and, if we include the asymmetrical body, the whole might be compared to a very deep, one-sided bell, with its narrower end half filled up. The single, sigmoid-arcuate, rigid *flagellum* arises from the depths of the bell, exactly at the middle of the truncate front, as it were forming a prolongation of the longitudinal axis of the body. There is no lip, and the *flagellum*, which rises close to the mouth, has a strong resemblance to that of *Monas*, both in proportion, form, and habits; and performs the office of a prehensile organ when the body is fixed, or acts as a propeller during natation. The *contractile vesicles* are two, or even three, in number, and lie in the posterior third of the body. The only species of this genus which I know of is gregarious in habit, but usually not more than four or five bodies are to be found attached, like *Anthophysa*, by their narrower, posterior ends, to the branchlets of a single forking stem. The peculiarity in regard to the direction of the curvature of the *flagella* in a backward direction, toward the stem, is as highly marked in *Codosiga* as in *Anthophysa* (described by me in the September number of this Journal) and there is also the same fixed relationship of the longitudinal and the greater and less transverse diameters of the several individuals of the colony.

There is still another new genus which I would like to mention here because it forms a collateral link with *Codosiga* in the affiliation of the Sponges with the *Monadina*. This genus I have called *Salpingœca*. It is, as it were, a single individual of *Codosiga* which does not possess a stem, but is seated in a *calyx*, from which it protrudes, or into which it retracts, at will. There are three well marked species, of which one is marine.

I come now to the principal object of this communication. The sponge which formed the main basis of these investigations is the well known marine species *Leucosolenia* (*Grantia*) *botryoides* Bowerbank. It is preëminently a branching form, and, on account of the slenderness and transparency of its tapering, hollow ramules, is a most desirable object for study. A branchlet—and in fact the whole colony—may be stated to be essentially a double tube. The outer tube consists of a glairy, gelatiniform stratum in which the spicules are imbedded in a certain order, and is pierced by numerous ostioles, which are continued through the interior tube to its hollow center. The inner layer, or tube, is entirely made up of the individual members of the colony, the bodies of which are packed together closely, side by side like pavement stones, with their posterior ends slightly imbedded in the glairy substance of the outer tube, and their anterior ends projecting freely into the general cavity. To describe the shape and organization of one of these individuals would be to repeat, almost word for word, what I have already said of the monad of *Codosiga;* in short *Leucosolenia* bears some such sort of relationship to *Codosiga* that *Salpingœca* does; the latter being as it were a stemless *Codosiga* seated in a calyx, whilst *Leucosolenia* is comparable to a stratum of the monads of *Codosiga* imbedded in a spiculiferous envelope. It is clear therefore that the organic difference between *Leucosolenia* and *Codosiga* is scarcely enough to locate them in two different families; in fact I am inclined to consider them only as generically distinct, and hardly, if at all, more widely separated in this respect than are *Salpingœca* and *Codosiga.*

What are the diversities of other genera of the *Spongiæ Ciliatæ* I cannot more than conjecture; but seeing that one of the genera is so closely related to the monociliate *Flagellata*, it can hardly be possible that the others are very far removed, and I shall feel warranted, therefore, in assuming, upon the premises, that the whole group of *Spongiæ Ciliatæ* is as intimately allied with the monociliate *Infusoria Flagellata* as is possible for it to be without actually constituting, with the latter, a uniform family.

ART. XLIV.—*Caricography;* by Prof. C. DEWEY.

Continued from vol. xlii, p. 331, 1866. (No. 44.)

Index to the Species.

THE description of the species of Carex of our country in this Journal was especially designed to aid those who had just entered upon the study of our plants. It was begun in 1824, only seven years after the great impulse, produced by the lectures of Professor Amos Eaton, to the study of botany and some other branches of natural history. There were few works on botany accessible to students; and, even when I had in 1815 ascertained the principles of the Artificial System of Linnæus and studied the genera in a general botany, no work describing the species was accessible. The Gramina of Dr. Muhlenberg was published in 1817, and his Catalogue of our plants a little earlier. These, with Persoon's Synopsis, the Reedgrass of Christian Schkuhr, and a Botanical Dictionary, were the works of which Eaton made such valuable and extensive application. The standard authors on Carices then were Schkuhr and Muhlenberg; and they were implicitly followed, even in the few mistakes they made, and for the correction of which no method occurred for many years. As knowledge of the species increased, the difficulties became perplexing, especially in relation to *Carex crinita* and *C. paleacea*, *C. oligocarpa* and *C. digitalis*, *C. folliculata* and var. *xanthophysa*, *C. plantaginea* and *C. anceps*. While American botanists have solved many of the difficulties, far more has been effected in the study of our North American Carices by Francis Boott, M.D., of the Linnæan Society, England, who enjoyed the greatest facilities and showed the most persevering activity.

In the Index, the quoted authorities are necessarily few. The name of the species, or the *first* name (when more are given), is considered the designation *due* to the species; the synonyms are in *italics*. The references, besides Schkuhr and Muhlenberg already mentioned, are chiefly the following:

Monograph of N. Am. Cyperacea, by John Torrey, M.D., published in 1838, and his Botany of the State of New York, 1843. The reference is Mon. 1836, or Tor. 1836 or 1843, or both.

Flora Boreali-Americana, Michaux, 1803. Though earlier published, it was of little use to us, till the result of Dr. Torrey's Examination of the Herbarium of Michaux in Paris, was published in vol. xxvii, 1835.

Carices Am. Septen. Exsiccatæ; edidit H. P. Sartwell, M.D., 1848. Pars I et II. This collection of Carices, not figures, 158 in number, nearly all correct, and fine specimens, is very interesting. The reference is Sart. or Exsic. 1848.

Prof. Tuckerman's Enumeratio Methodica, 1843, scientific, discriminating and curious, has some important references.

The reference, Carey 1848, is to the species of Carex described by Mr. Carey, in Dr. Gray's Manual of Botany, 1st ed., 1848.

Dr. Boott's publications were more numerous. 1. In Hooker's Flora Bor.-Americana, 1840; 2. On a species of Carex, allied, &c. Trans. Lin. Soc., 1843; 3. Six new N. Am. species of Carex, in Boston Journal of Nat. Hist., 1845; 4. De Caricibus, in Hooker's Journal of Botany, 1846; 5. Caricis species Novæ vel minus Cognitae, Lin. Soc., 1851; 6. Illustrations of the Genus Carex, Part I. 1858; Part II, 1860; and Part III, 1862. These are severally quoted as Boott 1840, Boott 1851, &c. Boott's Illustrations of Carex is a magnificent work containing full descriptions and splendid figures of 291 species on 126 pages and 411 plates, forming three large folio volumes. It is proper to add that Part IV was nearly complete for the press, when this admirable man and botanist was removed by death, on Christmas morning, 1864: born in Boston, 1792, and died in London. With noble generosity too he distributed the "Illustrations" among his numerous botanical friends.

Figures of 119 species (numbered 117) are given in several volumes, ending vol. xlix, 1845. There are in vol. ix, 12 figures; vol. x, 11 figures; xi, 26 figs.; xii, 2; xiv, 5; xxv, 3; xxvi, 2; xxviii, 6; xxix, 18; xxx, 9; xliii, 4; xlviii, 8; and xlix, 12 figures. Many of these are finely characteristic; but want of space prevents more particular reference.

The following notices are important:

1. A Catalogue of one hundred and twenty-eight species in vol. xi, pp. 319–325, 1826, as then understood. Due attention was not paid to *priority* of names.

2. Species of Carex in the Herbarium of Dr. Muhlenberg, as named by him, in possession of the Am. Phil. Soc., Philadelphia, compared with the species then current, vol. xxv, pp. 142–6, 1834.

3. Carices collected in Arctic America by the English Explorers, with the current names; vol. xxvii, pp. 270–5, 1835. Some changes are to be found in the index.

4. Determination by Dr. Torrey of the Carices in the Herbarium of Michaux at Paris, vol. xxvii, p. 276, 1835. The two or three mistakes, resulting from others in part at least, were corrected by him in 1843 or by others afterwards; and the changes are found in the present volume, some in the index.

5. Some notices of *varieties*, in the forty-four papers of thirty-six volumes, are not in the index; and some of the earlier synonyms, as then understood, may not all have been corrected.

Finally. These results are respectfully submitted to the lovers of this difficult genus and its multitude of species. The additions and changes time will unfold. To the six hundred species (or more) of Carex, many more will probably be added in the coming twenty years.

INDEX.

Carex adusta, Boott 1840 (not Carey).
— *argyrantha*, Tuckerman,[1] 1859; this Jour., vol. xxix, p. 346, 1860; xli, 331, 1866.
C. æstivalis, Curtis 1840;[2] iv, 347, 1847.
C. affinis, R. Br. 1823; xi, 305, 317, 1826.
C. alata, Tor. 1836; xxviii, 231, 1859;[3] Sart. No. 77.
— *straminea*, Sart. Exsic. No. 48 (not Schk.)
C. alopecoidea, Tuckerman Enum. 1843; xli, 326, 1866.
var. sparsi-spicata, Dew., viii, 350, 1849; xliii, 92, 1842.
— *cephalophora* var. *maxima*, Dew., xliii, 92, 1842.
C. alpestris, Allion, vii, 268, 1824; European.
var. tripla, Dew., xxxv, 59, 1863.
C. alpina, Vahl., xxvi, 377, 1834.
C. amplifolia, Boott 1840; iv, 345, 1847.
C. ampullacea, Good. 1792; vii, 266, 1824.
C. angustata, Boott 1840; xli, 326, 1866.
— *acuta*, Muh. (not Lin.), x, 265, 1826; Tor. 1836 and 1843.
C. aperta, Boott 1840; iv, 344, 1847.
— *acuta* var. *erecta*, Dew., x, 265, 1826.
C. aquatilis, Wahl., x, 267, 1826.
C. arctata, Boott 1840, and Illust., No. 124, 1858.
— *sylvatica* (not Huds.), x, 40, 1826.
C. arida, Schw. and Torrey, 1824.
— *Muskingumensis*, Schw. 1823; x, 281, 1826.
C. aristata, R. Br. 1823 (not Dew.); xi, 161, 1826, and xxviii. 240, 1835; xlix, 48, 1845, the Note is wrong.
var. longo-lanceata, Dew., xviii, 102, 1854.
C. atrata, Lin., x, 27, 1826.
var. ovata, Boott 1862; C. ovata, Rudge 1803; x, 44, 1826.
— *C. nigra*, Allion.; also Fries, Anderson and Boott.
C. aurea, Nuttall 1818; x, 48, 1826.
— *pyriformis*, Schw. 1823; ix, 69, 1825.
C. Backii, Boott 1840; xlix, 46, 1845.
C. Baltzellii, Chapman 1845; iii, 355, 1847.
C. Barbarac, Dew., xxxi, 24, 1861: doubtful.
C. Barrattii, Schw. & Torrey 1824; xi, 162, 1826, and xii, 297, 1827; Boott Illust. No. 176, 1858.
C. Bella-villa, Dew., xli, 229, 1866.
C. bicolor, All., xi, 306, 1826.
C. Boottiana, Bentham 1845; xxiv, 47, 1857.
C. bromoides, Schk., viii, 264, 1824.
C. Buckleyi, Dew., xlviii, 143, 1845.
— *Gebhardii*, Buckley (not Schk.).
C. bullata,[4] Schk. 1806; ix, 71, 1825 (not Carey, Boott, &c.).
— var. *cyindracea*, Dew., xi, 315, 1826: cancelled.
C. Buxbaumii, Wahl. 1803; x, 39, 1826; xlii, 245, 1866.
C. Canadensis, Dew., xli, 229, 1866.
C. capillaris, Lin., xxxix, 51, 1840.
C. capitata, Lin., xxxix, 51, 1840.
C. Careyana, Dew., xxx, 60, 1836; xxxv, 59, 1863.
C. Carltonia, Dew., xxviii, 238, 1835, and xli, 326, 1866.
C. cephaloidea, Dew., xli, 326, 1866; Boott, Illust. No. 285, 1858.
C. cephalophora, Muh. 1806; vii, 269, 1824, and x, 268, 1826.
C. Cherokeensis, Schw. 1823; xi, 160, 1826.
C. chordorrhiza, Lin., xlix, 44, 1845.
C. comosa,[5] Boott 1840; ix, 71, 1825.
C. *compacta* (not R. Br.), xxvii, 273, and xxviii, 237, 1835; cancel, lost.
C. compacta, R. Br. 1823.
— *membranacea*, Hooker (not Hoppe), xxix, 247, 1836.

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[1] Prof. Tuckerman, in xxxiv, 292, 1862, cancels this name.

[2] Described by Prof. Gray in xlii, 28, 1842.

[3] The caption should read ("Continued from vol. xxvii, p. 81," &c.), and the numbers should be changed from 254, 255 and 256 to 257, 258 and 259.

[4] The description and figure of C. bullata, Schk., are too unlike those of the plant so named by Dr. Boott, 39, 1858.

[5] Substitute this name for that of the species, ix, 71, 1825, that is, for C. pseudocyperus there.

C. concinna, R. Br. 1823; xi, 152, 1826.
C. concolor, R. Br. 1823; xi, 309, 1826.
C. conjuncta, Boott 1858; xxxix, 69, 1865.
— *vulpina*, Sullivant (not Lin.), viii, 348, 1849.
C. conoidea Schk. (not Muh.), x, 47, 1826.
— *granularioides*, Schw. 1823; ix, 262, 1825.
— *Illinoiensis*, Dew., vi, 245, 1848; diseased var.
C. Crawei, Dew., ii, 246, 1846.
var. heterostachia, Dew., ii, 248, 1846.
C. crinita, Lam. 1789; xxvii, 80, 1859; Muh. in part.
var. *paleacea*. *C. paleacea*,[6] Wahl., x, 270, 1826.
C. cristata, Schw. 1823; x, 44, 1826.
C. crus-corvi, Shuttleworth, 1832; Kunze, p. 128, 1850.
— *sicaeformis*, Boott, Boston Journal, 1845.
— *Halei*, Dew., ii, 248, 1846.
— *hystrix*, Gray, and C. *ornithorhyncha*, Fendler.
C. cryptocarpa, Meyer 1830; v, 173, 1848.
— *Scouleri*, Tor. 1836; Boott 1858, No. 158.
C. curta, Gooden. 1792; viii, 93, 1824.
— *Richardi*, Mx. 1803.
— *loliacea*, Dew. (not Lin.), xi, 306, 1826.
C. dascycarpa, Muh. 1817; xi, 148, 1826.
C Davalliana, Smith 1800? xlii, 244, 1866.
C. Davisii, Schw. & Torrey, 1824.
— *aristata*, Dew., vii, 277, 1824 (not R. Br.).
— *Torreyana*, Dew., x, 47, 1826.
C. debilis, Mx. 1803; *C. tenuis*, Rudge 1804.
— *flexuosa*, Schk. 1806; x, 40, 1826.
var. fusiformis, Dew., vi, 244, 1848, and xlii, 244, 1866.
C. decidua, Boott 1846; xxvii, 78, 1859, and xxxi, 26, 1861.
C. decomposita, Muh. 1816; xxv, 140, 1834.
— *paniculata* var. *decomposita*, Dew., x, 275, 1826.
C. Deweyana, Schw. 1823; ix, 62, 1825.
C. digitalis, Willd., xi, 147, 1826.
— *oligocarpa*, Muh. 1817 (not Schk.); x, 280, 1826; Tor. 1836 and 1843.
— *oligocarpa* var. *Van Vlechii*, Dew., x, 281, 1826.
— *Caroliniana*, Buckley[7] 1843; xlviii, 142, 1845.
C. dioica, Lin., x, 283, 1826.
C. disperma, Dew. 1820; viii, 266, 1824.
— *tenella*, Carey, after Carey & Boott (not Schk.[8] or Ehrht.), xix, 256, 1855.
C. disticha, Huds., xli, 330, 1866.
— *intermedia*, Gooden., iv, 343, 1847.
var. Sartwellii, Dew., xliii, 90, 1842, and xli, 330, 1866.
C. Douglasii, Boott 1840; xxiv, 46, 1857.
var. densi-spicata, Dew., xxxii, 41, 1861.
C. eburnea, Boott 1840.
— *alba*, Duv. (not Schk.), vii, 266, 1824, and xi, 316, 1826.
C. Elliottii, Schw. & Tor. 1824; *C. fulva*, Muh. (not good).
— *Baldwinia*, Dew., xxvi, 107, 1834.
C. elongata, Lin., iv, 345, 1847.
C. Emmonsii, Dew., in Torrey, Mon. 1836.
— *Davisii*, Dew., x, 279, 1826.
C. Emoryi, Dew., xxxi, 23, 1861.
C. exilis, Dew., xiv, 351, 1828.
var. squamacea, Dew., same page.
C. extensa, Gooden. 1792; xli, 327, 1866.
C. festiva, Dew., xxix, 246, 1836.
C. festucacea, Schk., viii, 96, 1824; Sart. Nos. 44 and 49, 1848.
— *fœnea*, var. 4, Boott 1862.
— *straminea*, var. festucacea, Boott 1862.
C. filifolia, Nuttall 1818; xi, 150, 1826, and xii, 296, 1827; xli, 329, 1866.
C. filiformis, Gooden. 1792; vii, 268, 1824.
C. flaccosperma, Dew. 1846; corrected, see C. xanthosperma.
C. flava, Lin., ix, 65, 1825.

[6] The mistake of Wahl. and of Schk. is corrected, xxvii, 79, 1859.

[7] Buckley, this Journal, xlv, 173, 1843.

[8] In fact there is no C. tenella, Schk.; for Schk. *himself adopted C. loeliacea* L. in *place* of his name *tenella*.

C. flexilis, Rudge 1803; *C. castanea*, Wahl.
— *blepharophora*, Gray, xxx, 59, 1836.
C. Floridana, Schw. 1823; x, 45, 1826.
C. foenea, Willd., x, 284, 1826, and xxv, 142, 1834.
C. folliculata, Lin. (not Schk. & Muh.).
— *xanthophysa*, Wahl., vii, 274, 1824, and xiv, 353, 1828.
C. formosa, Dew., viii, 98, 124.
C. Franklinii, Boott 1840; xxxii, 38, 1861.
— *ovata*, (not Rudge,) xxvii, 273, 1836.
C. Fraseri, Sims, Bot. Mag.; xi, 155, 1826.
C. fuliginosa, Schk. 1806; xi, 152, 1826.
— *misandra*, R. Br. 1823; xi, 153, 1826; Boott.
C. fulva, Gooden. 1792 (not Muh.)
— *binervis*, Smith, xxx, 61, 1836.
C. fulvicoma, Dew., xxix, 249, 1836.
C. Gayana, Desvous 1847? xlii, 244, 1866.
C. Geyeri, Boott 1846; xxvii, 78, 1859.
C. gigantea, Rudge 1803; xi, 164, 1826, and xli, 329, 1866.
C. glabra, Boott 1860; xxxix, 282, 1865.
C. glareosa, Wahl., iv, 344, 1847.
C. glaucescens, Elliott 1824; xi, 150, 1826.
— *verrucosa*, Muh. 1817, var. androgyna,[9] Curtis 1843; xlviii, 140, 1845.
C. gracillima, Schw. 1823; viii, 98, 1824.
C. granularis, Muh. 1817; vii, 272, 1824, and xi, 156, 1826.
C. Grayi, Carey[10] 1847; xxxv, 58, 1863.
C. grisea, Wahl. 1805.
— *laxiflora*, Schk. 1806 (not Lamarck); x, 31, 1836.
C. gynandra, Schw. 1823; xxvii, 79, 1859.
— *crinita* var. *gynandra*, x, 270, 1826; Muh. in part.
C. gynocrates, Wormsk., xxviii, 232, 1859.
— *dioica* var. *Davalliana* (not Wahl.), x, 283, 1826.
C. Halei, Carey (not Dewey), Boott Illust. No. 232, 1860.
— *turgescens* (not Torrey), iii, 356, 1847.
C. Halseyana,[11] Dew., xi, 313, 1826, and xxviii, 231, 1859.
— *polymorpha*, Muh. (not Schk.) in part; Boott 1858.
C. Hartii, Dew., xli, 226, 1866, and var. Bradleyi, 1866.
C. Hartwegii, Boott 1842; xlii, 244, 1866.
C. Haydenii, Dew., xviii, 103, 1854, and xxxv, 60, 1863.
C. Heleonastes, Lin., iv, 345, 1847.
C. hirsuta, Willd., ix, 260, 1825, and xi, 315, 1826.
C. Hitchcockiana, Dew., x, 274, 1826; xxxv, 58, 1863.
C. Hoodii, Boott 1840; xlix, 42, 1845.
C. Hookeriana, Dew., xxix, 248, 1836.
C. Hopneri, Boott 1840; xlix, 46, 1845.
C. Houghtonii, Torrey 1836; xxx, 63, 1836, and xxxix, 73, 1865 (not Sart. 130).
C. hystricina, Willd., x, 35, 1826.
— *Cooleyi*, Dew., a var., xlviii, 144, 1845.
— *Georgiana*, Dew., a var., vi, 245, 1848.
C. ignota, Dew., viii, 348, 1849.
C. incurva, Lightfoot, xxvi, 376, 1834, and xxxii, 39, 1861.
C. intumescens, Rudge 1804; x, 33, 1826.
— *folliculata*, Schk. (not Lin.), x, 32, 1826, and Muh. 1817.
C. Jamesii, Torrey 1836; v, 173, 1848, and xxxv, 60, 1863.
C. juncea, Willd., xli, 327, 1866.
— *miser*, Buckley, xlviii, 141, 1845, and xxix, 346, 1860.
C. Kneiskernii, Dew., ii, 247, 1846.
C. lævi-conica, Dew., xxiv, 47, 1857, and xxxv, 60, 1863.

[9] Description by Dr. Curtis in this Journal, xliv, 84, 1843.

[10] Mr. Carey's description in iv, 22, 1847.

[11] C. Halseyana is one of the three distinct species, imperfectly described, under C. polymorpha, Muh. (not Schk.); C. striata, Mx., is another of them; and the third is not yet ascertained. No one of them can, with propriety, be named C. polymorpha, Muh., because the name does not designate any one. This is acted out by Dr. Boott in Illust., No. 56, 1858, as named by him, "C. polymorpha (Muhlenberg) (C. Halseyana, Dewey)." Dr. Boott evidently saw that C. polymorpha, Muh., did not designate the species, and hence added C. Halseyana, Dew., which did show the species intended. C. polymorpha, Muh., *in part*, is even too bad when avoidable.

C. lævigata, Smith, Brit. Flora.
— *Greeniana*, Dew., xxx, 61, 1836.
C. lagopina, Wahl., v, 172, 1848.
C. lagopodioides, Schk., viii, 95, 1824.
C. lanceata, Dew., xxix, 249, 1836.
C. languinosa, Mx. 1803.
— *pellita*, Muh. 1806; ix, 70, 1825.
C. laxiflora, Lam. 1789 (not Schk.); xxvii, 80, 1859.
— *anceps*, Muh., x, 36, 1826.
  var. patulifolia, Dew., and var. angustifolia, Dew., 1845.
C. Leavenworthii, Dew., ii, 246, 1846.
C. leiocarpa, Meyer 1830; v, 174, 1848.
C. lenticularis, Mx. 1803 (not Dewey); v, 175, 1848; xxviii, 232, 1859, and xxix, 348, 1860.
C. lepidocarpa, Tausch, iii, 172, 1847; var. of C. flava, L.
C. leporina, Lin., xxvii, 272, 1835.
— *ovalis*, Good. 1792; vii, 276, 1824, and xxvi, 277, 1834.
C. leucoglochin, Ehrht., x, 42, 1826.
C. Liddoni, Boott 1840; xlix, 45, 1845.
C. limosa, Lin., x, 41, 1826.
— *lenticularis*, Dew. (not Mx.), vii, 273, 1824.
— *laxa*, (not Wahl.) xxvi, 376, 1834.
  var. radicalis, Paine, xxxix, 71, 1865.
C. livida, Willd., and var. radicalis, Paine, xli, 329, 1866.
— *limosa* var. *livida*, Wahl. 1803; x, 41, 1826, and xli, 329, 1866.
— *Grayana*, Dew., xxv, 141, 1834.
C. longirostris, Tor., ix, 257, 1825.
C. lupulina, Muh. 1806; xi, 165, 1826.
  var. 2, gigantoidea, Dew. 1865; xli, 328, 1866.
C. lupuliformis, Sartwell, ix, 29, 1850.
— lupulina var. polystachya, Schw. & Torrey 1824; xi, 166, 1826.
C. lucorum, Willd., xix, 252, 1855. Doubtful.
C. Lyoni, Boott 1840; xlix, 42, 1845.
C. Macounii, Dew., xli, 228, 1866.
C. macrocephala, Willd., xliii, 91, 1842.
C. macro-chaeta, Meyer 1830.
— *spectabilis*, Dew., xxix, 248, 1836.
C. Magellanica, Lam. 1789; xxxix, 70, 1865, and Boott 1860.
— *limosa* var. *irrigua*, Wahl. 1803; x, 41, 1826.
— *paupercula*, Mx. 1803 and Torrey 1843; xlii, 330, 1866.
— *irrigua*, Smith 1845; x, 42, 1826.
C. marcida, Boott 1840; xlix, 43, 1845.
C. marina, Dew., xxix, 247, 1836.
C. maritima, Vahl., iv, 343, 1847.
C. media, R. Br. 1823; xi, 309, 1826.
C. Meadii, Dew., xliii, 90, 1842; Boott 1858.
C. Mertensii, Prescott 1833?
— *Columbiana*,[12] Dew., xxx, 62, 1836.
C. microdonta, Torrey & Hooker 1836; v, 174, 1848, and xxxv, 60, 1863.
C. microglochin, Wahl., v, 174, 1848.
C. microstachya, Mx. 1803 (not Erhrt, earlier), Mon. 1836.
— *polytrichoides*, Muh. 1806; ix, 258, 1825.
C. miliacea, Muh., x, 30, 1826.
C. miliaris, Mx. 1803; x, 36, 1826, and xli, 330, 1866.
C. mirabilis, Dew., xxx, 63, 1836.
C. mirata, Dew., xxxix, 71, 1865.
  var. minor, Dew., same date; doubtful.
C. Mitchelliana, Curtis,[13] xlviii, 140, 1845.
C. monile, Tuckerman, xxix, 346, 1860; Exsic. 152, 1848, probably true.
C. Muhlenbergii, Schk., viii, 265, 1824.
C. muricata, Lin., xi, 307, 1826.
C. mutica, R. Br. 1823; xi, 310, 1826, and xxix, 252, 1836.
C. nardina, Fries 1838? iv, 246, 1847.
— *Hepburnii*, Boott 1840.
C. Nebraskensis, Dew., xviii, 102, 1854, and xxxv, 60, 1863.
C. nigricans, Meyer 1830; xxix, 249, 1836.

[12] Named before Prescott's work had come to hand, as in some other cases.
[13] See his description, this Journal, xliv, 84, 1843.

C. nigro-marginata, Schw. 1823: x, 282, 1826.
C. Novæ-Angliæ, Schw. 1823; ix, 64, 1825.
— *collecta*, Dew., a var., xi, 314, 1826.
C. Norvegica, Wahl., xxxii, 38, 1861.
C. Nuttallii, Dew., xliii, 92, 1842.
— *Meekii*, Dew., a var.; xxiv, 48, 1857.
C. obtusata, Lilj.
— *Backana*, Dew., xxix, 259, 1836.
C. Oederi, Ehrht. 1800; x, 38, 1826, and xlii, 245, 1866.
— *viridula*, Mx. 1803; xi, 153, 1826, and xlii, 245, 1866.
C. oligocarpa, Schk. (not Muh.) 1806; iv, 349, 1847.[14]
  var. Sartwelliana, Dew., v, 176, 1848.
C. oligosperma. Mx. 1803; xi, 160, 1826.
— *Oakesiana*, Dew., xiv, 351, 1828.
C. Olneyi, Boott 1858; xxix, 347, 1860.
C. oxylepis, Tor. & Hook. 1836; iii, 354, 1847.
C. pallescens, Lin., vii, 267, 1824.
C. paludosa, Good. 1792; xxix, 346, 1860.
C. panicea, Lin., xxv, 140, 1834.
C. paniculata, Lin., x, 275, 1826, for its varieties.
C. paradoxa, Schk., iv, 346, 1847.
C. Parryana, Dew., xxviii, 239, 1835, and xlii, 331, 1866.
— *arctica*, Dew., the same, *young*.
C. pedunculata, Muh. 1806; ix, 259, 1825.
C. Pennsylvanica, Lam. 1789; xix, 252, 1855.
— marginata, Muh. 1806, xi, 163, 1826; var. Dew., xxxv, 59, 1863.
C. petasata, Dew., xxix, 246, 1836.
C. petricosa, Dew., xxix, 246, 1836.
C. physema, Dew., xxix, 347, 1860.
— *bullata*, Carey 1848, or Boott 1858 (not Schk.).
C. plantaginea, Lam. 1789, Muh. & Schk. in part; vii, 272, 1824, and xi, 155, 1826.
C. platyphylla, Carey,[15] viii, 349, 1849.
C. podocarpa, R. Br. 1823; xi, 162, 1826, and xxix, 251, 1836.
C. præcox, Jacq.
— *verna*, Vill., xi, 314, 1826.
C. pseudo-cyperus, Lin., iv, 348, 1847.
C. pubescens, Muh., ix, 73, 1825, and xli, 330, 1866.
C. pyrenaica, Wahl., iv, 346, 1847.
C. Raeana, Boott 1851 and 1858; xli, 230, 1866.
C. rariflora, Smith 1845; xxxix, 71, 1865.
— *limosa* var. *rariflora*, Wahl. 1803; x, 42, 1826.
C. Raynoldsii, Dew., xxxii, 39, 1861.
C. recta, Boott 1840; v, 175, 1848.
C. Redowskiana, Meyer 1830; xxix, 250, 1836.
C. remota, Lin., xi, 309, 1826.
C. retrocurva, Dew. 1845; xlii, 243, 1866.
— oligocarpa, Muh. (not Schk.), var. latifolia, Gray 1835.
C. retroflexa, Muh. 1806; vii, 271, 1824, and x, 277, 1826.
C. retrorsa, Schw. 1823; ix, 67, 1825.
C. Richardsoni, R. Br. 1823: xi, 152, 1826.
C. rigida, Good. (not Carey), xlix, 45, 1845.
— *saxatilis*, Fl. Dan. Oeder 1764; xi, 310, 1826 (not Lin.).
C. riparia, Curtis 1792; xlix, 47, 1845, and xxxv, 60, 1863.
— *lacustris*, Willd., x, 43, 1826; united, Tor. 1836.
  var. laxiflora, Dew., xxxv, 60, 1863.
C. rosea, Schk., x, 276, 1826.
  var. radiata, Dew., same reference.
C. Rossii, Boott 1840; xxxv, 57, 1863.
C. rostrata, Mx. 1803 (not Schk. or Muh.); xxxix, 52, 1840.
— *xanthophysa*, Wahl., var. *nana* and *minor*, Dew., xiv, 358, 1828; xxxix, 73, note, 1865.
C. rotundata, Wahl. 1803; xli, 327, 1866.
C. rupestris, Allion.
— *attenuata*, R. Br. 1823; xi, 305, 1826.
— *Drummondiana*, Dew., xxix, 251, 1836.
C. salina, Wahl. 1803; iv, 344, 1847.
C. saxatilis, Lin. 1737; xxviii, 236, 1835.

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[14] This description overlooked by Dr. Boott in his No. 93, 1858.
[15] Description by Mr. Carey, in iv, 23, 1847.

— *pulla*, Gooden. 1792; Boott 1843; Fries 1846.
C. scabrata, Schw. 1823; ix, 66, 1825.
C. scabrior, Sartwell, viii, 349, 1849.
C. Schkuhrii, Willd., xxviii, 238, 1835.
— *supina*, Willd., xxvi, 376, 1834.
C. Schottii, Dew., xxxi, 25, 1861.
B. Schweinitzii, Dew., ix, 68, 1825.
C. scirpoidea, Mx. 1803; xi, 154, 1826.
— *Wormskioldiana*, Hornm, xiv, 352, 1828; xi, 154, 1826.
C. scirpoides, Schk., viii, 96, 1824.
C. scoparia, Schk., viii, 94, 1824.
C. setacea, Dew., ix, 61, 1825.
C. Shortiana, Dew., xxx, 60, 1836; C. Schortii, Tor., later.
C. siccata, Dew., x, 278, 1826.
— *pallida*, Meyer 1830.
C. Sitchensis, Prescott 1827?
— *cryptocarpa*, (not Meyer,) xxix, 245, 1836.
C. sparganioides, Muh., viii, 255, 1824.
var. minor, Boott 1862.
— *muricata* var. *cephaloidea*, Dew., xli, 330, 1866.
C. squarrosa, Lin. 1757; vii, 270, 1824.
var. typinoides, Dew., xi, 316, 1826.
C. stellulata, Gooden. 1792; xi, 306, 1826.
C. stenolepis, Tor. 1835; xxx, 59, 1836, and xli, 331, 1866.
C. stenophylla, Wahl. 1803; xxviii, 237, 1835.
C. sterilis, Willd., xi, 304, 1826.
C. steudelii, Kunth 1835; xlix, 46, 1845.
C. stipata, Muh., vii, 271, 1824.
C. straminea, Willd., vii, 276, 1824, and xi, 157, 1826.
var. brevior, Dew., xi, 158 and 318, 1826.
— var. aperta, Boott 1862, and C. tenera, Olney.
C. striata, Mx. 1803; xxviii, 231, 1859.
— *polymorpha*, Muh., iii, 255, 1847, and xlii, 329, 1866, note.
— *Houghtonii*, Sart. No. 130 (not Torrey).
C. striatula, Mx. 1803; xxvii, 276, 1835, and xlii, 245, 1866.
— *anceps*, in part Am. authors; Mon. 1836.
— *blanda*, Dew., x, 45, 1826; Torrey 1843.
— *conoidea*, Muh. (not Schk.) 1817.
C. stricta, Gooden. 1792 (not Lam.); x, 269, 1826.
C. styloflexa, Buckley[16] 1843; xlviii, 141, 1845.
C. stylosa, Meyer 1830; xlii, 243, 1866, and xxix, 252, 1836.
C. subulata, Mx. 1803 (not Wahl.).
— *Collinsii*, Nuttall 1818; xi, 317, 1826.
— *Michauxii*, Dew., x, 273, 1826; as subulata, used by Wahl.
C. Sullivantii, Boott[17] 1840; xlix, 44, 1845.
C. sychnocephala, Carey.[18]
— cyperoides (not Lin.), iii, 171, 1847.
C. tenax, Chapman, xix, 254, 1855; Sart., 113, 1848.
C. tenella, Ehrhart 1800? (not Schk. or Carey); xxviii, 232, 1859.
— *canescens* var. *alpicola*, Wahl.
var. *sphærostachya*, Tuck. En. 1843.
— *sphærostachya*, Dew., xlix, 44, 1845.
— *vitilis*, Fries 1846; Anderson 1849.
— *canescens* var. *vitilis*, Carey 1848.
— *Persoonii*, Sieb., xix, 253, 1855.
C. tenera, Dewey, viii, 97, 1824.
C. tentaculata, Muh., x, 34, 1826.
var. gracilis, Boott, 231, 1860.
var. rostrata, Sartwell (not Schk. or Mx.), Exsic. 1848.
var. parvula, Paine, Cat., 157, 1865.
C. tenuiflora, Wahl., xxxix, 51, 1840.
C. teretinscula, Gooden. 1792; vii, 265, 1824.
C. tetanica, Schk. (not Muh.), xi, 312, 1826.
C. Thurberi, Dew., xxxi, 24, 1861, and xxxv, 60, 1863.
C. Torreyi, Tuck. 1843; xlix, 43, 1845.

[16] Description by Mr. Buckley in this Journal, xlv, 174, 1843.
[17] Description by Dr. Boott in xlii, 39, 1842.
[18] Mr. Carey's description in iv, 24, 1847.

torta, Boott 1843; ix, 30, 1850.
cæspitosa, (not of Lin. or Gooden.), x, 266, 1826.
var. ramosa, Dew., xii, 297, 1827.
acuta, in part (not Lin.), x, 265, 1826, and var. sparsiflora, Dew.
triceps, Mx. 1803; xlviii, 142, 1845.
trichocarpa, Muh. 1806; vii, 274, 1824, and xi, 158, 1826.
var. turbinata, Dew., xi, 159, 1826.
triquetra, Boott 1845; xxxv, 60, 1863.
- *monticola*, Dew., xxxi, 26, 1861.
trisperma, Dew., ix, 63, 1825.
Tuckermani, Dew., xlix, 48, 1845, and Boott 1846.
turgescens,[18] Tor. 1836; xxxv, 57, 1863.
umbellata,[19] Schk. 1806; x, 31, 1826, not xxxi, 26, 1861.
var. vicina, Dew., xi, 317, 1826.
ursina, Dew., xxviii, 240, 1835.
ustulata, Wahl., xi, 149, 1826, and Schk.
utriculata, Boott 1840; xxviii, 231, 1859.
var. sparsiflora, Dew., xxviii, 232, 1859.
- *cylindrica*, Schw. 1823 (not Tuck. or Carey); xli, 331, 1866.
vaginata, Tousch 1821; xli, 227, 1866; Kunze 1850.
var. alti-caulis, Dew., xli, 227, xlii, 245, 1866.
Vahlii, Schk. 1806; xxvi, 377, 1834.
vallicola, Dew., xxxii, 40, 1861.
varia, Muh.[20] 1806; xi, 162, 1826.
var. pedicellata, Dew., xi, 163, 1826.
C. Vaseyi,[21] Dew., xxix, 347, 1860, and xli, 331, 1866.
— *monile*, Sartwell (not Tuckerman), xlix, 47, 1845.
— *vesicaria* var. *cylindracea*, Dew. 1845.
C. venusta, Dew., xxvi, 107, 1834.
C. verrucosa, Schw. (not Muh.), xi, 159, 1826; mistake of Schw., and is expunged.
C. verticillata, Boott 1858; xli, 230, 1866.
C. vesicaria, Lin., x, 273, 1826; Boott 1846.
C. vestita, Willd., ix, 261, 1825.
C. virescens, Muh., ix, 259, 1825.
C. vulpinoidea, Mx. 1803.
— *multiflora*, Muh. 1806; ix, 60, 1825.
var. microsperma, Dew., xi, 317, 1826.
C. vulgaris, Fries 1846, Nov. Mant.
— *cæspitosa*,[22] Gooden. 1792, & Muh. (not Lin.), xii, 297, 1827, last of the note.
C. Washingtoniana, x, 272, 1826, and xii, 296, 1827.
— *rigida*, Carey (not Gooden.) in Gray, 1848.
C. Wildenovii, Schk., ix, 258, 1825, and xi, 311, 1826.

[18] C. turgescens, iii, 356, 1847, should be named C. Halei, Carey (not Dewey).

[19] In the description of this species I added to that of Schk. and Muh. the characters of the form *vicina*, which they had never seen, and thus I introduced under their name *umbellata*, a plant with other characters which their language did not admit, and yet I called it by their name. In the next volume I separated this *vicina*, characterizing it as *var. vicina* of C. umbellata, Schk. I thought the truth required this, that I might give to Schk. and Muh. their right.

[20] On the principle of the last note, when I found plants closely resembling C. varia, Schk. & Muh., except not having *their sessile* spikelets, I named them C. varia, Muh., var. pedicellata, as these were not found by Muh. till long after Schk. had published C. varia. By blending the characters, the *var.* becomes the *type*, and the *type* of Muh. becomes the *variety*, as in Boott, Illust., No. 239, 1860.

[21] Recent examinations show that C. Vaseyi is probably only an immature form of C. monile, Tuck., as Dr. Sartwell named it in his No. 152, Exsic.; yet both Dr. Boott and Prof. Tuckerman rejected it. As the locality has been destroyed, Dr. S. cannot obtain mature specimens there for proof.

[22] C. cæspitosa, Gooden., an English Carex, was confounded with, or itself absorbed, C. cæspitosa, Lin., from 1792 till they were shown to be distinct and were separated by Dredger, as Dr. Boott states, in 1841. As the former is found in New England, and widely over the north and west, it is placed under the name given it by Fries, as above, C. vulgaris, to prevent mistakes for the Linnæan name.

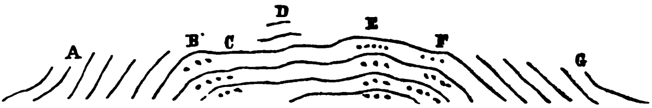
C. Woodii, Dew., ii, 249, 1846, and iv, 349, 1847.
— *titanica*, Muh. (not Schk.), iv, 349, 1847.
C. Wrightii, Dew., xxxi, 24, 1861, and xxxv, 60, 1863.
C. xanthosperma,[23] Dew., ii, 245, 1846.
— *flacco-sperma*, Dew. 1846, and Boott 1858.
C. xerocarpa,[24] S. H. Wright, xlii, 334, 1866, note.

ART. XLV.—*On the Oil-producing Uplift of West Virginia;* by Prof. E. W. EVANS.

THE most productive oil region of West Virginia, so far as developments have yet gone, is confined to a line of uplift, extending from Burning Springs on the Little Kanawha, in a direction from twelve to fourteen degrees east of north, to the Ohio river,—a distance of 35 miles. Thus limited, the line is nearly bisected by the Northwestern Virginia railroad, at Petroleum station; which will therefore serve as a convenient point of reference.

In its central portion, which for convenience I shall designate as the middle segment, the upheaval is most marked, and exhibits some distinctive features. This part is about ten miles in length, and is also nearly bisected by the railroad. A cross-section of it, as made out from the railroad cuttings, the records of borings, and the frequent exposures of rock along the streams of the neighborhood, is shown in fig. 1. It is a decapitated fold,—a flexure of the strata in the anticlinal form, having the summit worn down by water.

1.



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[23] This name should be substituted for *flaccosperma* in the volume and page here given, as the name was selected on account of the *yellow* color of the fruit on the plants received from the south: *yellow fruited* Sedge. C. flaccosperma is an accidental miscopy.

[24] 304. *C. xerocarpa*, S. H. Wright. Spikes 4–6, long, very slender erect; the staminate 2–4, sessile or sub-pedicelled, with close ovate acute scales; the pistillate 1–2, close fruited except the lower very lax part, the upper half staminate; stigmas 2; fruit ovate short-acute, small and close, compressed and apiculate, longer than ovate acute scale, or equalling it, except some of the upper and long lanceolate scales; culm 18 to 24 inches, erect, slender, 3-sided and scabrous on the upper part; bracts leaf-like; leaves slender, narrow, scabrous on the edge and shorter than the culm; plant, except the dry brownish *arid* spikes and fruit which give the name, dark green.

Prattsburg, Steuben Co., N. Y., on a rich peat bottom in an extensive flat, abundant over one-fourth of an acre, without any other Carex.—*Dr. S. H. Wright.* Of the *stricta* species, this is the *strictest*, with the longest small spikes and close smallest fruit.

There is a steep western slope, from A to B, where the rocks are tilted up at an angle varying from 45° toward B to upwards of 60° toward A. The distance across these upturned edges, from A to B, measured horizontally is about 1200 feet. After allowing for the divergent dips, I estimate the original thickness of rock here represented to be about 850 feet.

In the eastern steep slope, from F to G, the strata incline at an angle of from 30° to 40°; but the distance across, taken horizontally, is so much greater as to render the thickness of rock thus displaced about the same as in the western slope.

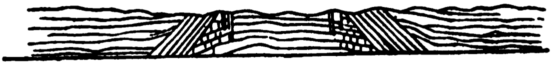
To the westward from A, and to the eastward from B, there is a somewhat sudden transition from the steep slopes to a comparatively slight inclination of the strata, which continues far enough to either side to give an additional outcropping of about 300 feet, estimated perpendicularly to the strata.

It is not claimed that these figures are accurate; but if they are approximately correct, the rocks on the summit of the anticlinal have been brought up out of place not less than 1100 feet, probably as much as 1200 at the highest axial point. This estimate has reference to the place of maximum upheaval, a little north of the railroad.

In the inner belt between the steep slopes, which is about a mile in breadth, from B to F, the strata are nearly horizontal, but somewhat wavy—the waves partaking of the direction of the main uplift. One of these, represented at E, is quite prominent and persistent, and in fact constitutes the crown or proper axial line of the anticlinal. It is much nearer the eastern slope than the western.

The most distinctive features of this segment of the uplift are seen in the abrupt change of direction at B and F. The Appalachian folds farther east, where there was more heating and crystallization, exhibit curves rather than angles, even when most prominent. But here we find sharp angular points, with some arching to either side, but upon the whole partaking rather of the character of fractures, and affording numerous crevices in the soft and brittle rock. Hence the popular name of "the break," by which this uplift is known, is not altogether inapplicable. There is, however, no actual disruption of the strata, and no fault, either at the western angle B, or at the eastern angle F. The abruptness of these angles has led some into the

2.



supposition that the nearly horizontal strata of the inner belt are not continuous and identical with the inclined strata to either side, but abut against their sides or faces, in the manner exhibited in fig. 2. That such is not the case is conclusively shown by the following facts.

First, a comparison of the rocks in the western slope, from B to A, fig. 1, by their lithological characters, with the rocks in the eastern slope, from F to G, shows that they are identical one by one in their order; they are also found identical with the rocks in the intermediate hills, as at D, so far as these extend upward, say a maximum height of about 300 feet. The strata thus traced over include sandstones, shales and veins of coal; also a layer of flinty limestone, which may be mentioned as one of the best guides. At the railroad this appears on the west side projecting upward through the bed of Laurel Fork, and on the east side in an exposure of sloping rocks in the hill a little south of Petroleum station. At various places it not only appears in the opposite slopes, but has also been traced through the hills between, as on Comb run, about two miles south of the railroad. Indeed there are numerous exposures in situations corresponding to B and F, fig. 1, where the continuous connection of the same strata, as they change from an approximately horizontal position to a steep pitch, is plainly traced by the eye.

Again, it is a fact that the rocks bored through in positions a little to the left of B, fig. 1, correspond, in lithological characters, with those found to the right of it, but differ from the latter in being struck further down, and in continuing longer in proportion to a greatly increased inclination. Borings made at B descend into rocks of steep pitch. All this is in direct contradiction to the hypothesis presented in fig. 2, where the sloping rocks are represented as not only distinct from, but also as projecting over the horizontal ones.

The inner belt between the slopes, from B to F, is not inappropriately called the "oil belt," as it is only in the horizontal or slightly inclined rocks between these limits that oil has been found in paying quantities. In the upturned edges of the east and west slopes, as well as over a considerable extent of territory outside, a large number of wells have been bored for oil, but with little or no success.

Even within the inner belt, the producing wells are thus far nearly all confined to three narrow strips. It will be seen that the cross section, fig. 1, exhibits three principal angles, marking three distinct axial lines; the western angle at B, which is the most prominent, next the eastern angle at F, then the inner angle at E, which is the least prominent. It is along these three axes that all the good wells are located.

The most successful developments are along the western margin of the inner belt, on a strip about 500 feet in width, from B to C, fig. 1. On the eastern branches of Laurel fork, on White

Oak fork, and other small streams from one to four miles north of the railroad, and on the intervening hills, a great many wells have been sunk on this line; and a majority of them are yielding oil, in some cases from 100 to 300 barrels per day, but generally from 10 to 100.

The oil is usually found in a brittle light-gray or white sandstone, or group of sandstones, from 150 to 350 feet below the level of the streams; the depth varying somewhat, however, with different localities. Crevices are sometimes struck at a less depth, especially on the side toward B; but they are generally dry crevices, or contain only water. As the principal line of fracturing (given the fragility of the rock) would follow the axis plane, approximately bisecting the angle at B, it would of course incline to the eastward from B in its descent, as represented in fig. 1. Accordingly the most frequent strikes, and generally the best wells, are not on the side next to the slope, but on the eastern half of the strip, toward C, where this main line of crevices, in its eastern ramifications, would cross the above-mentioned sandstone, after it has become horizontal.

On the eastern margin of the inner belt, the main if not the only developments are on Gales' fork of Myers' creek, about three and a half miles north of the railroad, where there are some good wells, confined to a similar narrow strip, just west of F; but the production is not so large, in proportion to the number of wells sunk, as on the western margin.

On the inner axis, at E, and a short distance to either side of it, there have been numerous strikes on Oil Spring run, from one to two miles north of the railroad, and on Hughes river near the California House, about four miles south of the railroad; but the yield of oil at these localities is small.

Thus it is seen that the quantity of oil found bears something like a direct ratio to the amount of fracturing. Many borings have been made in the areas that lie between the three productive strips, and a few of them, located on minor flexures, have been partially successful. Deep boring will probably reduce the barren strips to narrower limits, but the one or two deep wells that have been already sunk, between the western and inner angles, are too far off to approach the region of the axis-plane of either.

This large accumulation of oil in the crevices of the anticlinal would seem to be owing, not solely to a direct connection by a continued line of fractures with the original sources of the oil in strata beneath, but in part also to the collection, from a wider area, of oil that has come up elsewhere, as through the crevices of the adjacent synclinals. For, being lighter than water, it would gradually work up between the strata of the slopes. Where these are decapitated it has escaped to the surface; but the inner (or lower) strata of the slopes conduct to crevices still covered by a mass of horizontal rock, where oil is now found.

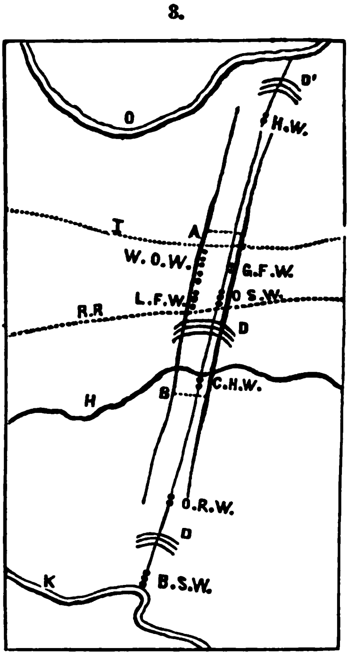
The surface rocks of the country adjacent to this uplift, on either side, are well known to be those of the Upper Coal-measures. The lowest rocks brought to the surface by the upheaval are undoubtedly those of the Lower Coal-measures. This is indicated by the occasional appearance of Lepidodendra. In the hills between the slopes there are three or four veins of coal, and their equivalents are found in the inner strata of the slopes; but where the upheaval has been greatest, I think only two seams have been detected in wells sunk in the valleys;—those are very thin, and both occur over 100 feet below the surface. From 150 to 500 feet down (in approximate figures) the rocks are mainly white sandstones with some conglomerate and salt water. This, or the main part of it, is the conglomerate or millstone grit series, which in West Virginia is mainly a sandstone, and constitutes the principal salt rock. From 500 to 850 feet down there is found a series of light blue shales, interstratified, toward the bottom, with some thin layers of conglomeratic sandstone. This, or the main part of it, is probably the upper group of Subcarboniferous rocks (the Umbral of Rogers), which in Pennsylvania and the northern parts of West Virginia is composed chiefly of shales, but to the south and west becomes calcareous. Below 850 feet is a sandstone which has not yet been penetrated a great distance. This may be the Lower Subcarboniferous (or Vespertine); or, as from data furnished by Rogers we may infer that the latter is but slightly developed in this region, and may be represented by the thin conglomerates just mentioned, it is probable that the sandstone belongs to the Chemung group, overlying the black shales, and constituting the geological horizon of most of the oil obtained in Pennsylvania.

The opinion has been expressed, in this Journal, that a well 860 feet deep, put down near the middle of the inner belt, in the neighborhood which we are considering, and at a point where it is represented that a depth of about 1000 feet from the top of the Upper Coal-measures down has been eroded, passes down through the lowest Carboniferous rocks, then through the Waverly sandstone (or supposed equivalent of the Chemung group), then through 265 feet of the equivalents of the Black shales, far down in the Devonian.

To this hypothesis there are strong objections. It makes the strata included between the top of the Upper Coal-measures and the Black shales much thinner in the aggregate at this point than they are estimated, in the Ohio reports, to be in the neighboring counties to the northwest of the Ohio river. This is in direct contradiction to the general fact, as set forth by Rogers and others, that there is a thickening of these strata on going southeastward toward the mountains. Especially does it seem necessary to allow an appreciable thickness at this point to the upper or shaly group of the Subcarboniferous, which, according to Rogers, has a much more extensive distribution westward, from its eastern outcrop in the Appalachian chain, than the lower or sandstone group. I am aware that at the northwestern outcrops in Ohio it is scarcely, if at all, represented. But its increase to the southeast is so great, attaining as it does a thickness of 3000 feet at the mountains, that it is not difficult to suppose it has a thickness of 300 feet at this point,—especially as at the Great Kanawha salines eight miles above Charleston, a point not very much nearer its line of maximum development than this, it is found in boring to have a thickness of not less than 700 feet. Besides, the 300 feet of shales which I have set down as the Upper Subcarboniferous bear very little resemblance to the highly bituminous group known as the Black shales. They are not black, neither are they bituminous. The occasional occurrence of streaks of dry carbonaceous matter which, as it comes up with the fine borings and floats on the water, resembles powdered charcoal, and is known as "soot," does not distinguish these shales from the Carboniferous strata overlying them. The theory suggested, that they have been decolorized by heating and debituminization, hardly comports with the fact that among the eastward Appalachian ridges, where the heating has been so much greater than here that the coal is debituminized, and the black shales themselves not only debituminized but metamorphosed into slates, they still retain their characteristic dark coloring of carbonaceous matter. If anything more were wanting to show that the group above referred to is not the equivalent of the Black shales, it would seem to be supplied by the fact that the rock underlying them is found to be sandstone, and not limestone, as that hypothesis would require.

Thus far I have confined my remarks chiefly to the middle segment of the uplift, with which I am personally best acquainted. I am indebted for many important facts, especially in regard to the northern and southern segments, to Col. A. J. Warner, a practical geologist who has repeatedly traversed the country the whole length of this line, for the purpose of exploration.

As we follow it northward toward the Ohio river, and southward toward the Little Kanawha, the upheaval becomes less marked, and assumes a somewhat different character. The eastern and western axes become less and less prominent, and the middle axis becomes more prominent by comparison, though really subsiding; and this finally becomes the sole axis, toward both extremities of the line. At the northern and southern ends of what I have designated as the middle segment, a little north of the Northwestern Virginia turnpike, and a little south of Hughes river, the subsidence of the eastern and western axes becomes somewhat sudden and marked. Both however can be clearly traced a few miles further, northward across the headwaters of Bull creek to within five or six miles of the Ohio river, and southward across the headwaters of Standing Stone creek to within seven miles of Burning Springs. The western axis seems to extend a little further, both ways, than the eastern.



B. S. W.—Burning Spring Wells.
C. H. W.—California House Wells.
D—Dips.
G. F. W.—Gales Fork Wells.
H—Hughes river.
H. W.—Horseneck Wells.
K—Little Kanawha river.
L. F. W.—Laurel Fork Wells.
O—Ohio river.
O. R. W.—Oil Rock Wells.
O. S. W.—Oil Spring Run Wells.
R. R.—Northwest Virginia Railroad.
T—Northwest Virginia Turnpike.

It has been asserted that the middle portion of this uplift so narrows down toward both ends that the opposite slopes actually come together, thus enclosing an insular space, in the form of a double convex lens. A careful examination will show that such is not the fact; so far from it that toward their extremities the eastern and western axes actually diverge, as one after another of the inner strata of the slopes sink to a lower inclination.

Both at Burning Springs and on the Ohio, the cross section exhibits only a simple anticlinal or flexure, whose axis is the prolongation of what has been called the middle axis. Strictly speaking, these are not the extreme points of the uplift; it has been traced in a diminishing wave some miles across both rivers; and probably its whole length is as much as 50 miles. It is replaced, toward both ends, by other and minor flexures, approximately parallel with it.

At nearly all points along this uplift where a thorough test has been made, on the lines before indicated, oil has been found in good quantity. The principal developments on the southern segment are at Burning Springs on the Little Kanawha, and at

Oil Rock on the headwaters of Standing Stone creek. These are on or near the main axial line. Some deep wells recently sunk yield as much as 500 barrels per day. On the northern segment there are some producing wells in a similar position on Horseneck, about three miles from the Ohio river.

The annexed cut, fig. 3, showing approximately the course and relative positions of the three axes, and the locations of producing wells, will, for the most part, explain itself. A and B are the extremities of the middle segment. To avoid crowding, the smaller streams have been omitted, and the intervals between the axes enlarged.

The oil found in the middle segment is mostly heavy or lubricating oil; that found in the northern and southern segments is mostly light oil. A probable explanation of this is afforded by the theory that the heavy and less volatile oil is a residuum, remaining after the evaporation and drainage of the lighter oils, through crevices connecting with the surface. The angles being more abrupt in the middle segment, and affording more open crevices, the escape of the lighter oil would be more rapid. Another fact of similar significance is, that even here light oil is often found along the inner or least abrupt of the three angles. This is the case at the California House, and, to a less extent, on Oil Spring run.

The wells of the middle segment obtain their supplies mainly from the upper part of that group of sandstones which we have considered as representing the conglomerate series. The deep wells at Burning Springs obtain light oil from the same geological horizon; but the shallow wells of the northern and southern segments derive their oil from the rocks of the Coal-measures. A deep well near the railroad, in the middle segment, which has penetrated over 100 feet into the probable equivalents of the main oil rocks of Pennsylvania, yields, from the bottom, an immense quantity of carburetted hydrogen but no petroleum.

On this as well as other anticlinals in this region, burning springs are of quite common occurrence, but they are in most cases locally associated with light oil. Many of the wells from which light oil is obtained afford gas enough to throw the oil and water above the surface. In some cases this takes place on the fire-engine principle, the compressed gas being confined and the liquids alone escaping; in other cases the escaping gas forces out before it whatever liquids it meets in its way.

The tendency to break into square or rhombic blocks, so as to afford an extensive system of vertical fissures, would seem to be one of the determining characters of the main oil rocks. This feature belongs to several of the coarse sandstones of the Coal-measures; but it is most conspicuously seen in the conglomerate at its various outcrops, as in Hocking county, Ohio, and at the falls of the Great Kanawha;—so also near the head of the Alleghany river, where the groups of regular blocks, standing out upon the surface, have given rise to such names as Rock City and Ruined City. In some cases, however, the oil-yielding rock is found crushed into small fragments.

This line of uplift is approximately parallel with the Appalachian folds to the eastward, and is undoubtedly a member of the same system. The statement has appeared in this Journal that it makes an angle with the mountain chains of about 40°, —an error arising probably from comparing it with the mountains of Tennessee or northern Pennsylvania, instead of with those which lie in a lateral direction from it; namely, in northern Virginia. The latter constitute one of those segments of the Appalachian zone where the mountains, according to Rogers, bear approximately north and south. This flexure is as nearly parallel with their general course as they are with one another. Nor does it stand isolated from the rest; for in the intermediate space there are several other minor flexures, having the same general direction, though occurring at intervals which increase to the westward.

As establishing the theory that the lateral pressure concerned in uplifting the Appalachian folds was exerted from the ocean side (as if by the subsidence of a submarine area in a period of great cooling and contraction), it has been shown by geologists that as a general fact the western dips of the folds are steeper than the eastern,—that upon going westward, further from the direct action of the moving force, this feature becomes less marked,—that to the westward also the folds are less crowded together, or separated by wider intervals, and the uplifting connected with less metamorphism and debituminization. This fold, as one of the westernmost members of the series, conforms to the rest in all these general laws which, with the fact of parallelism, serve to connect them with a common cause, as parts of one system.

Toward the extremities of this uplift, where it becomes a comparatively slight flexure, the greater inclination of its western dip ceases; but this forms no exception to the general fact as seen in the other Appalachian folds. As would follow from the above theory in regard to their origin, it is not until they attain a considerable angular elevation that the pushing over of their summits, which is a surface movement, begins to take place.

Here, as in other parts of the Appalachian region, there is evidence of a transverse system of disturbances. An example is seen on McFarland's run of Hughes river, a few miles east of this uplift, in a vertical fissure cutting through nearly horizontal rocks with a bearing, as described by Lesley, of 78° east of north. It is filled with a sort of solidified bitumen, or asphalt, —the result, probably, of the slow oxydation of the heavy residuum of oils which once occupied it.

Art. XLVI.—*Remarks on the Drift of the Western and Southern States, and its relation to the Glacier and Iceberg Theories;* by Eug. W. Hilgard, Ph.D., State Geologist of Mississippi.

In a recent paper on the Quaternary formations of Mississippi (this Journal, May, 1866), I have expressed my views concerning the remarkable formation designated as the "Orange Sand," in my Report on the Geology of Mississippi.

The admirable exposition of the whole subject of the Post-tertiary formations in Dana's Manual of Geology, to which I have since been enabled to refer, as well as the portion of Dana's Address before the American Association in 1855, relating to that period, suggest to me with greater clearness the points to be settled in establishing the presumed correspondence of the Orange Sand of the Southwest to the recognized drift of the Northwest. On this subject I now propose to offer a few additional remarks.

I have not perhaps, in the paper referred to, been sufficiently explicit, in comparing the Orange Sand to the northern [illegible] in general, without specially mentioning as its supposed congener, the drift of the Northwest, particularly that of Illinois, Wisconsin, Iowa and Missouri. That the drift of New England, as described by Hitchcock and Dana, can be satisfactorily accounted for on the glacier theory alone, few who have delved among the ancient and modern moraines of Switzerland will question.

But the Western drift deposits, even so far north as Iowa, as Hall's observations show, and still more in Illinois and Missouri, differ seriously in their structure both from the New England moraines, and from the lines of blocks apparently left by the glacier melted *in situ*. In the West the "erratic" blocks, both rounded and angular, are commonly found imbedded in deposits distinctly stratified over considerable areas, and consisting of gravel, sand, or even clay; and the pebbles accompanying them are not the irregular, scored and scratched forms of the moraine pebble, but clearly rounded by aqueous action.

Where a moraine has not, subsequently to its formation, been violently shoved forward by an unusual advance of the glacier, some semblance of stratification may occur, in consequence of great regularity in the rolling down of the detritus. But such an appearance rarely extends connectedly beyond a few yards, nor is it likely it ever should, however stupendous the scale of the glacier. Nor will any one, acquainted with moraines and their material, be likely to mistake an aqueous deposit for a moraine, or *vice versâ.*

In Desmoines county, Iowa, according to Hall, the drift consists of "partially stratified deposits of clay, sand and gravel, with boulders of primary and secondary rocks irregularly distributed through the mass, though usually most abundant in the lower portion." These boulders in the description of Lee county are spoken of as "worn and rounded masses" of various rocks, occurring in distant localities. We have the same in Henry county; in Van Buren, this deposit furnished "a mass of siliceous wood," which "presented none of the water-worn characters of a boulder; but the angles were as sharp and well defined as if it had never been removed from the spot where it was at first buried." In Washington county, again, we find mentioned "a heavy deposit of drift material presenting the usual characteristics of this formation, and consisting of irregularly stratified beds of sand, gravel and clay, with an average thickness of from forty to sixty feet."

The above, taken *verbatim* from Hall's Iowa Report, might serve as a very fair description of a good portion of the Orange Sand of Mississippi, the only difference being the greater size of the boulders in the more northern locality; a merely quantitative variation.

In Missouri the phenomena are the same. The drift there, according to Swallow, is "a heterogeneous stratum" (but a *stratum* still!) "of sand, gravel and boulders, all water-worn fragments of the older rocks." Some of these are derived from localities as far distant as "St. Peters river, about 300 miles north of St. Joseph. But the paleozoic fragments are usually from localities near where they rest.... and are as *completely rounded* as those from more distant points."

The latter remark is interesting, as it shows precisely what I found to be the case in the eastern pebble belt of Mississippi. The pebbles there are almost exclusively derived from the siliceous group of the Carboniferous, which occurs only in patches farther northward, but underlies the pebble strata themselves (Miss. Rept., p. 17, ff.).

But Swallow goes on to say that "there are other deposits, particularly in the middle and southern parts of the State" (*sic!*) "which are not genuine drift, and yet they bear a greater resemblance to that than to any other formation, and occupy the same stratigraphical position."

This, also, is precisely the predicament of the Orange Sand deposits.

As for the drift of Illinois, I am unable to refer to the Reports of that State; but my own early recollections of it such as it exists in St. Clair county, place it in precisely the same category with the drift of Iowa and Missouri. Only I remember distinctly the occurrence in it, of *angular* boulders of syenite, greenstone and quartzite.

Nevertheless, most of the material by far, in all these cases, is *water-worn;* it is, more or less irregularly but distinctly, *stratified; no glacier scorings* are mentioned, either on the pebbles or on the adjacent rock.

The *glacier theory alone* cannot, therefore, account for these deposits north of the Ohio, any more than for the Orange Sand delta south of it. And even as far south as Vicksburg, the action of *water alone* is inadequate to account for the transportation of the boulders found in these beds.

Dana's remark (Manual, p. 554) that "while the glaciers were disappearing, many a stream or lake would have existed to stratify the drift, and cause denudation in elevated places," points no doubt to the true explanation of these phenomena: but it does not go far enough to satisfy existing facts. Agassiz has observed that "the melting snows of the declining glacier epoch" may have been instrumental in the formation of the river terraces; but Tuomey was, I believe, the first to point out, that the Southern drift may have been formed in consequence of the *sudden* melting of the northern glaciers (Second Report on the Geology of Alabama, ed. Mallet, p. 146); such as would have resulted from a first, rapid depression of so huge a mass of ice below the snow line.

The assumption of a pretty *rapid* depression seems necessary to account for the immense volumes of water required to produce the observed effects, and also to explain the possibility of inland transportation of boulders by floating ice—if indeed, after a depression of near 500 feet below the present level in the latitude of Montreal, *inland* it could be called. Though comparatively but slightly depressed at its mouth, the more immediate valley of the Mississippi would have formed a deep irregular gulf, filled with ice-cold water by the enormous influx from the glacier region forming its northern shore; whose icebergs would thus be floated southward in a medium unfit for the permanent existence of any fauna likely to be fossilized, viz: ice-cold fresh water in a state of violent flow.

That the action must at first have been extremely violent, is proved by the deep erosion of the underlying formations, and the transportation and subsequent redeposition in mass, of their materials, more or less altered; which is exhibited on so extensive a scale in Mississippi. But for the fact that wherever the more ancient strata were readily susceptible of denudation, they have entered largely, at times almost exclusively, into the composition of the Orange Sand strata, we might suppose that the surface of the former (which appears to be at least equally as hilly as the present one) had been denuded by atmospheric agencies during the glacial period of elevation.

When, after the subsidence of the first rush, the velocity of the water had so far diminished as to render it capable of forming stratified deposits, these would naturally possess the mixed character resulting from a twofold mode of transportation—by water, and by floating ice—the former, no doubt, in many cases, succeeding the latter, and, by the grinding action of the smaller gravel and sand, transforming the angular blocks first dropped into the rounded boulders characterizing the drift of Iowa and Missouri, and faintly represented by their scattered congeners in the Orange Sand delta.

It would thus seem that the grandly simple means of a single elevation and re-depression in the northern latitudes, to which the phenomena of the ancient glaciers and sea-beaches point us, will equally satisfy the conditions required for the formation of the Western and Southern Drift. Down to the later stages of the northern re-depression, the predominant slope would everywhere be southward, so as to collect in the Mississippi valley the glacier waters, not only of the whole extent of northern territory now tributary to it, but probably also those of the present arctic slope of British America, and a portion of that now tributary to the St. Lawrence. Hence the comparative absence of stratified drift from the Northern Atlantic slope of the United States; and its presence, on the contrary, on the sea border of the Southern States, as stated by Tuomey, *l. c.*

As to the Champlain epoch, it would seem to be represented in its *later* part only, by that which in the Western and Gulf States has formed the "second bottoms." In the Mississippi Valley the formation of the latter has clearly been preceded by a period of comparative quiet succeeding the stormy times of the drift proper, and giving rise, successively, to the deposits of the Bluff or Loess, and the superincumbent Yellow Loam. These ought to be contemporaneous with the earlier stages of the Champlain epoch in New England and Canada; and their great difference of character might, it seems to me, be explained on the simple and probable supposition, that in the more southerly regions the depression took place somewhat later, even as, obviously, it was vastly less in extent. The Bluff formation of the valley of the Mississippi bears the most obvious relations to the present channels of the larger rivers, and it contains the vestiges of a fauna proving that the deluge of ice-cold water had ceased; while yet the volume of water carried by those channels greatly exceeded that which corresponds to the era of the second bottoms, south, or to the bottom prairie of Missouri.

The only sea-beach terrace now existing on the Gulf coast, and with which the second bottoms show a manifest confluence of level and material, is from 18 to 24 feet above tide-water. But the evidences of sea-beach or tidal action extend far back into the interior (Miss. Rept. p. 29), so that in fact, the second bottom of the Pascagoula exhibits *that* structure, and not the usual one resulting from flowing water, throughout its course. The same structure, moreover, extends to some elevation into the bordering uplands, where these sands overlie the Post-pliocene beds and Orange Sand.

The present beach terrace of the gulf coast cannot, therefore, be considered as the true measure of the amount either of depression during, or re-elevation subsequent to, the Champlain epoch. Whether the great absolute elevation of some of the Orange Sand ridges of Mississippi and Alabama (probably exceeding in some cases, 700 feet) necessitates the assumption of even a greater depression than the present beach-marks would indicate, the known facts are hardly adequate to determine.

University of Mississippi, July 12, 1866.

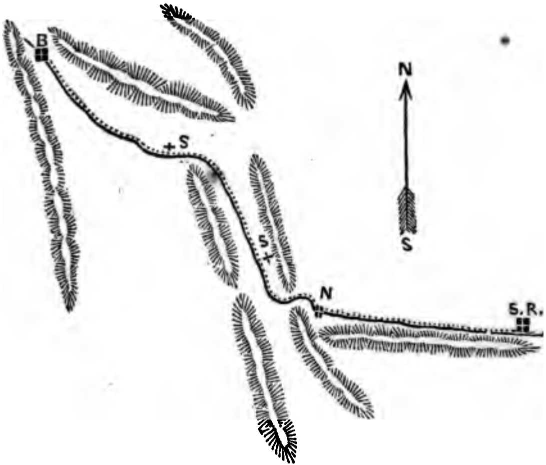
ART. XLVII.—*New locality of Meteoric Iron in Cohahuila, Northern Mexico;* by CHARLES UPHAM SHEPARD.

FOR my knowledge of this most remarkable locality I am indebted to the following communication of my friend Prof. Forrest Shepherd of New Haven, Conn., who, twenty-seven years ago, furnished me the earliest notice of the fall of a meteoric stone at Little Piney, Mo. The weightiness of the present contribution makes ample amends for so protracted a silence.

"*My dear Sir:*—Remembering your untiring efforts in meteoric research, I take pleasure in forwarding to you a specimen broken from the immense mass of native iron protruding from the earth in Northern Mexico, northwest from Santa Rosa, of the department of Cohahuila. This I am enabled to do through the kindness and perseverance of my esteemed friend Maj. E. M. Hamilton, who, with Col. David Branson and Capt. Charles W. Dey, accompanied me in my late visit to the silver mines of Nueva Leon, and to each of whom I am indebted for protection and numerous kind offices.

The route pursued by Maj. Hamilton was as follows (see map):

From Santa Rosa westerly to Naciemento, 40 miles, more or less. From Naciemento westerly or northwesterly about 15 miles to a gap through the mountains, called Puerta Santana. From the Puerta, northerly for 60 miles, along the valley, past a spring to the termination of the mountain on the left; and thence around the end of the mountain, passing a second spring, by a northwest route for a distance of about 50 miles toward an apparent junction of the mountains, and where the valley becomes narrow, to an open space about one-quarter of a mile square, partially overgrown by palmetto palms. Here might be seen fourteen ponderous masses of native iron, the largest of which rises upwards of four feet above the ground, having the form of a bee-hive—being five feet in diameter where it enters the soil, and into which it descends for an unknown depth. Major Hamilton informed me that he excavated on one of its sides to the distance of eighteen inches, without discovering any diminution in its diameter.



B, Bonanza; **N**, Naciemento; **P**, Puerta Santana; **S**, Springs; **S. R.**, Santa Rosa.

"Hoping that this remarkable locality may hereafter shed some additional light on the mysteries of aërolitic origin,

"I remain very truly yours, FORREST SHEPHERD."

As the above communication does not give any name for the locality, it seemed requisite to bestow one, out of regard to convenience of reference in our meteoric catalogues. I have therefore affixed to it the designation *Bonanza*, upon Prof. Shepherd's map.

I am unable to decide whether these iron masses have been described by others, though it may prove they are the same as those mentioned by Assistant A. Schott on page 34, part ii, of Major Emory's Report on the Mexican Boundary Survey, and which are laid down as occurring 90 miles northwest from Santa Rosa; but whether he refers to the Santa Rosa of Cohahuila, lat. 28°, long. 101° 30′, is not certain. They certainly do not seem to belong to the locality on the *Sancha estate*, from whence came the 250 pound mass in the Smithsonian Museum at Washington, as this is stated to have occurred but 50 or 60 miles from Santa Rosa, Cohahuila.

Description of the specimen.

It is a flattened cleavage mass rather above a quarter of an inch in thickness, by two inches in length and one and a half broad. It weighs 120 grams, or rather more than a quarter of a pound. Its color is dark iron-black without any appearance of iron-rust. About three-fourths inch square of one of its sides exhibits the original surface of the mass, showing three well marked though shallow polyhedral depressions, but, like the Madoc, the Texas, and many other irons, without any well defined crust. For the rest, the two broad surfaces are nearly plane, even and parallel, and evidently came from a natural cleavage in the mass. The edges, except where recently broken, are also plane, and have resulted from octahedral cleavages.

A very distinct cleavage shows itself also about midway between the two broad surfaces; and taking advantage of this through the slight projection of a lamina rather more than one-eighth of an inch in thickness, a fragment weighing six grams was easily broken with the aid of a vise and a few slight blows of the hammer. The separation brought to light the most perfect cleavage I have yet seen among meteoric irons, as also the most splendent and silvery luster. Experiments upon other portions of the mass made it likewise apparent that throughout, it belonged to one individual crystal, like similar fragments from the great Orange river, the Braunau, and the Putnam irons. No lamina of a large crystal of blende or fluor could present a more homogeneous or brilliant cleavage than belongs to this entire specimen; nor should I be surprised if in this respect, it is other than a microcosm of the parent block.

About half a gram was treated with dilute nitric acid. It was readily attacked, and after twenty minutes was dissolved, with the exception of a slight pulverulent or granular residue, which, on being cleared from the solution, washed and thrown upon a filter, was seen to consist of short, glittering, needle-like points, almost too minute to be discerned without the assistance of a lens. Among them was a feeble trace of a blackish powder, proceeding no doubt from the black pellicle of the exterior of the mass. The brilliant capillary points, when more nearly examined, seemed identical in size and character with the rhabdite crystals of Reichenbach, so beautifully figured in Prof. Rose's admirable memoir on meteorites,[1] as occurring in the Braunau meteoric iron. The lens also shows upon the cleavage planes of the iron, those peculiar striæ which Prof. Rose has pointed out as occurring in the Braunau iron; in particular, such as are parallel to the edges and certain diagonals of the cube, and which Prestel has shown to be present also in cleavage crystals of artificial iron. So far therefore as one sample can show, the structural identity of this iron with the interesting Braunau mass is very clear.

The specific gravity, determined by a single experiment, was 7·50. The solution in nitric acid, when precipitated by ammonia, gave the blue color characteristic of nickel.

I shall avail of an early opportunity for having a portion of the surface polished and etched; as also for making a fuller investigation in respect to its chemical constitution.

In this extraordinary occurrence of metallic meteorites, we are again forced to reflect upon the singular manner in which they follow great mountain chains, at least on the American continent. Is their preponderance in such regions at all explained by the greater impenetrability of the surfaces upon which they have fallen?

Amherst College, Sept. 14, 1866.

ART. XLVIII.—*On the Spectra and Composition of the Elements*; by Prof. GUSTAVUS HINRICHS, Iowa State University.

§ 1. Two years ago I communicated to this Journal the results of a *preliminary* investigation of the distribution of the dark lines in the spectra of some of the elements, especially the spectra of the alkaline earths and iron.[1] At that time no exact and comprehensive determinations of the *wave-lengths* corresponding to these lines had been made; our investigation was therefore necessarily based upon Kirchhoff's arbitrary millimeter scale. As shown by us, this scale will answer very well for a limited range of the spectrum—but if distant parts of the spectrum are to be compared, this scale becomes useless, and the absolute wave-lengths are required. We, for this purpose, made use of Fraunhofer's principal lines and a geometrical interpolation—but these points of comparison are evidently too few to warrant great accuracy.

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[1] Beschreibung und Eintheilung der Meteoriten auf grund der Sammlung im mineralogischen Museum zu Berlin, von Gustav Rose. Berlin, 1864.

[1] This Journal, 1864, vol. xxxviii, pp. 31–40.

Having now some direct determinations of wave-lengths, we resume our investigation. It will be seen that the results of our preliminary investigation are fully confirmed in their essential features, and that the number of elements to which they apply is greatly enlarged.

§ 2. The basis for the present investigation are the determinations of Plücker and Ditscheiner. The results only of Plücker's determinations are accessible to us;[2] they apply to the metalloids, thus enabling us to extend our investigations to these elements. Ditscheiner[3] has determined the wave-length of a great number of the lines laid down in Kirchhoff's map of the spectrum, thereby enabling us by a simple geometrical interpolation to find the wave-length of any of Kirchhoff's lines with great accuracy, the number of lines determined by Ditscheiner being sufficiently extensive to warrant the use of a large scale in this interpolation. The scale we used was 1 mm. of Kirchhoff's scale and one ten-millionth *millimeter wave-length*, severally equal to two millimeters. On this scale the length of the spectrum from B to G, or from 600 mm. to 2800 mm. Kirchhoff, taken as abscissæ, becomes 4400 mm., or nearly 15 feet. In some instances we used a five times longer scale, making the spectrum 75 feet.

There is still another series of determinations by Angström,[4] but the original memoir is not accessible to us, and the extract in this Journal does not admit of direct comparison with Kirchhoff's scale.

§ 3. We will now first give the extension of our laws to the spectra of the metalloids on the basis of Plücker's determinations; then review the spectra of the alkaline earths and iron by means of Ditscheiner's measurements; and in conclusion make a few theoretical remarks suggested in the course of our investigation.

In the following, W will always denote the *wave-length* expressed in *ten-millionths of a millimeter*, D the differences, i the assumed number of equal intervals, W′ the calculated wave-lengths, E the error, the difference between observation and calculation, $E = W - W'$ and *d the interval.*

§ 4. *Hydrogen spectrum.*

Line.	W	D	i	W′	E
α	6533	1690	10	6533	0
β	4843	504	3	4843	0
γ	4339			4336	+3

where $d = 169$. Range from red to indigo, or C to G,

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[2] This Journal, 1865, vol. xxxix, pp. 217–18.

[3] Bestimmung der Wellenlänge der Fraunhofer'schen Linien des Sonnenspectrum. Sitzungsberichte der Akad. d. Wiss. Wien, 1864, Bd. 50, pp. 296–341. This Journal, 1866, vol. xli, pp. 395–396.

[4] This Journal, 1865, xxxix, 215.

§ 5. *Chlorine spectrum.*

Line.	W	D	*i*	W′	E
α	5451	235	5	5451	0
β	5216	424	9	5216	0
γ	4792			4793	+1

where $d=47$. Range, from yellow to blue.

§ 6. *Bromine spectrum.*

Line.	W	D	*i*	W′	E
α	5169	376	15	5166	+3
β	4793	27	1	4791	+2
γ	4766	75	3	4766	0
δ	4691			4691	0

where $d=25$. Range, from green to blue.

It will be noticed that the first interval is five times the last.

§ 7. *Iodine spectrum.*

Line.	W	D	*i*	W′	E
α	?			?	
β	5947			5949	−2
γ	?	610	77	?	
δ	5337	170	21	5333	+4
ε	5167	506	63	5165	+2
ζ	4661	32	4	4661	0
η	4629	183	23	4629	0
θ	4446	231	29	4445	+1
ι	4215			4213	+2

where $d=8$. Range, from orange to indigo.

Though the differences E are not quite so insignificant as in the preceding, they are yet within the errors of observation; and though the intervals seem to follow no law, yet they may be grouped as follows:

$$\begin{array}{llllll} & 77 & 21 & 63 & \underbrace{4 \quad 23 \quad 29} \\ \text{or} & 77 & 21 & 63 & 56 \\ \text{or} & 11 & \underbrace{3 \quad\; 9} & & 8 \text{ times } 7 \\ \text{or} & 11 & 12 & & 8 \\ \text{very nearly} & & 3:3:2, & & \end{array}$$

and $4+23=27$ with 29 forms another group if an intermediate line at 28 should be observed. As it seems, 7 is divisible in most of these intervals, $7 . d=56$ will be an interval of a higher order.

§ 8. *Nitrogen spectrum.*

Line.	W	D	*i*	W′	E
β	6610	521	8	6609	+1
11	6089	327	5	6089	0
17	5762			5764	−2

where $d=65$. Range, from red to yellow.

). *Oxygen spectrum.*

Line.	W	D	i	W′	E
α	6150	822	6	6150	0
β	5328	143	1	5327	−1
γ	5185	818	6	5190	+5
δ	4367			4367	0

·e $d=137\frac{1}{6}$. Range, from orange to near indigo.

10. *Mercury spectrum.*

Line.	W	D	i	W′	E
α	5782	23	1	5784	−2
α′	5759	298	12	5759	0
β	5461	102	4	5459	+2
γ	4359			5359	0

·e $d=25$. Range, from yellow to near indigo.

ı the synopsis of Plücker's determinations given in this Jour-· (vol. xxxix), we find also the lines of several compounds, we will improve this opportunity to test the application of law of multiple distances to the four binary compounds in·igated by Plücker.

11. *Terchlorid of phosphorus spectrum.*

Line.	W	D	i	W′	E
α	6493	469	13	6492	+1
β	6024	433	12	6024	0
γ	4591			4592	−1

re $d=36$.

12. *Bichlorid of silicon spectrum.*

Line.	W	D	i	W′	E
α	6329	358	3	6329	0
β	5978	028	8	5980	−2
γ	5050			5050	0

re $d=116\frac{1}{3}$.

13. *Bichlorid of tin spectrum.*

Line.	W	D	i	W′	E
α	6445	651	65	6444	+1
β	5794	210	21	5794	0
γ	5584	251	25	5584	0
δ	5333	809	81	5334	−1
ε	4524			4524	0

re $d=10$.

14. *Carbonic acid spectrum.*

Line.	W	D	i	W′	E
γ	5599	409	41	5600	−1
δ	5190	689	69	5190	0
ζ	4501	119	12	4500	−1
η	4382			4380	+2

ıre $d=10$.

§ 15. The preceding tables may be condensed to the following:

Substance.	*d*	Values of *i*.					
Hydrogen,	169	10	3				
Chlorine,	47	5	9				
Bromine,	25	15	1	3			
Iodine,	8	77	21	63	4	23	29
	or	77 :	84	:		56	
	or	11 :	12	:		8	
	or	$2\frac{1}{12}$:	3	:		2	
Nitrogen,	65	8	5				
Oxyen,	$137\frac{1}{6}$	6	1	6			
Mercury,	25	1	12	14			
Terchlorid of phosphorus,	36	13	12				
Bichlorid of silicon,	$116\frac{1}{3}$	3	8				
Bichlorid of tin,	10	65	21	25	81		
Carbonic acid,	10	41	69	12			

§ 16. It can hardly be assumed that the determinations of Plücker are as accurate as those of Ditscheiner; the latter are, as will be shown, not always reliable in the last figure. Hence the agreement between observation and our calculation is in most cases astonishingly close; thus for hydrogen, where $d=$ 169 there is only one single deviation, and this amounts to only three units; the chlorine spectrum shows as good as no deviation at all, only one unit for $d=47$ on a range of 700. It is similar for bromine, oxygen, nitrogen, and mercury. Only iodine deviates for one single line (δ) more than is proper, especially as d is only 8; but then this being the only instance, it may be due to a greater error in the observation of δ.

It may perhaps be objected, that only few lines are taken for each element. But we have taken *all* lines for which determinations could be found, and though they may be few in number, they are ranging through the greater part of the spectrum.

Assuming that the other metalloids will correspond in this as they do in all other respects, we may express the above by the following law: *the wave-lengths of the dark lines in the spectra of the several metalloids differ from one another by simple multiple of a certain number, d, peculiar for each element*, and the spectra of the binary compounds given do not show so simple multiples, but the differences are yet expressible in multiples of a certain number for each compound.

The above law is substantially the same as the laws deduced in the preliminary investigation.

§ 17. In all the foregoing tables we have made use of Plücker's observations, which are only known to us in their final result. The following values of W will be taken from Ditscheiner, and as his memoir is at hand, we may first ascertain the degree of accuracy of his observations.

For Kirchhoff's line 1834·0 he finds W=5040·0 from 13 determinations, ranging from 5039·3 to 5041·4, showing a range of 2·1 (page 326, l. c.).

For K=1655·6 he finds W=5165·8 from 13 determinations, ranging from 5165·1 to 5166·8 or a range of nearly two of our units (page 325).

In accordance herewith we found it impossible for small parts of the spectrum, to draw a straight line through the points determined according to Ditscheiner's wave lengths as ordinates to Kirchhoff's millimeters as abscissæ; showing that the decimal of Ditscheiner could not be relied upon, and in a few instances even the unit was uncertain.[1] We will now review the *calcium* spectrum, as given in our first article. In addition to the preceding, I will signify the intensity as given by Kirchhoff, and K the place on Kirchhoff's millimeter scale. W is obtained as before stated, either directly from Ditscheiner's table, if the line itself was measured by him, or by geometrical interpolation, having carefully constructed the neighboring part of the spectrum—always making a separate drawing for each group of lines according to the scale mentioned in § 2. It was impossible to obtain reliable results by arithmetical interpolation, the small errors of observation thereby affecting the final result to a very great extent, while by proper drawing of the line the individual errors of observation were as good as eliminated.

§ 18. *Calcium spectrum.*—We will, as in our first article, consider each of the six principal groups separately.

GROUP I. $d_1=3·3$. Near line G.

I	K	W	D	*i*	W′	E
5c	2869·7	4322·0	13·2	4	4322·0	·0
4b	2864·7	4308·8	6·5	2	4308·8	·0
4	2854·7	4303·2	3·3	1	4302·2	+·1
4c	2834·2	4299·0			4298·9	+·1

The intervals are just as given in the preliminary investigation—and E is completely within the errors of observation.

Group II contains too few lines.

GROUP III. $d_2=1·16$. Near line E.

I	K	W	D	*i*	W′	E
4b	1533·1	5261·5			5261·48	+·02
4b	1532·5	5261·7	2·1	2		
4c	1530·2	5263·6	1·3	1	5263·80	—·20
5c	1528·7	5264·9	4·7	4	5264·96	—·06
6c	1522·7	5269·6			5269·6	·00

The scale used for this interpolation was three times the one given in § 2; 1 mm. K was represented by 6 mm.

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[1] In order to prevent misunderstandings it must be remembered that our unit is the ten-millionth of a millimeter, while Ditscheiner expresses his results in millionths of a millimeter.

The difference between the first two lines is too small to warrant any conclusion drawn from the same.

GROUP IV. $d_4 = 1·46$. In the yellow.

I	K	W	D	i	W′	E
3d	1235·0	5582·0	5·7	4	5582·00	·00
4c	1229·6	5587·7	1·7	1	5587·84	−·16
2d	1228·3	5589·4	4·2	3	5589·30	+·10
5d	1224·7	5593·6	3·9	2	5593·68	−·08
5d	1221·6	5597·5	2·5	2	5596·60	−·10
3c	1219·2	5600·0	1·0	1	5599·52	+·42
5d	1217·8	5601·0			5600·98	+·02

These intervals form the following simple series:

$$4 \qquad \underbrace{1 \qquad 3} \qquad 2 \qquad 2 \qquad 1$$

$$\text{or} \quad 4 \qquad 4 \qquad \underbrace{2 \qquad 2} \qquad 1$$

$$\text{or} \quad 4 \qquad 4 \qquad 4 \qquad 1$$

GROUP V. $d_5 = 6·57$. In the orange.

I	K	W	D	i	W′	E
2e	894·9	6102·0	19·0	3	6102·0	·00
4b	884·9	6121·0	40·0	6	6121·71	−·71
5b	863·9	6161·0	6·7	1	6161·13	−·13
3d	860·2	6167·7			6167·70	·00

GROUP VI.

There are too few observations for a very accurate determination of the corresponding wave-lengths of this group in the red part of the spectrum. The difference of 2 3 K as found in our preliminary investigation corresponds to 6·23 wave-length in this part of the spectrum.

Comparing the different groups we need not expect the difference *d* itself to be the same for all groups; but such a multiplum thereof as the intervals indicate, ought to be the same. Thus we find

Group I, $d_1 = 3·3$ $\qquad 2d_1 = 6·60$
" III, $d_3 = 1·16$ $\qquad 6d_3 = 6·96$
" IV, $d_4 = 1·46$ $\qquad 4d_4 = 5·84$
" V, $d_5 = 6·57$ $\qquad 1d_5 = 6·57$
" VI, $d_6 = 6·23$ $\qquad 1d_6 = 6·23$
the mean of which is $\qquad d = 6·44$

As far as the above numbers go, we see that the interval approaches equality, but is not strictly so. Yet it may well be borne in mind that the greatest deviation either above or below the given mean value amounts to only half a unit; so that though the equality of the interval cannot be considered as demonstrated, yet the inequality is not demonstrated either.

It is now necessary to consider the single lines given by Kirchoff outside of the preceding groups. These give the following esult: $d=16$.

I	K	W	D	i	W′	E
2*c*	1832·8	5041·0	144·6	9	5041·0	0·0
5*b*	1627·2	5185·6	159·6	10	5185·0	+·6
2*b*	1443·5	5345·2	511·1	32	5345·0	+·2
3*c*	1029·3	5856·3	864·9	54	5857·0	−·7
2*b*	641·0	6721·2			6721·0	+·2

Introducing the groups, we obtain, always $d=16$.

Group.	I	K	W	i	W′	E
I.	5*c*	2869·7	4322·0	7	4321·0	+1·0
II.	5*c*	2638·8	4434·0	38	4433·0	+1·0
	2*c*	1832·8	5041·0	9	5041·0	0·0
	5*b*	1627·2	5185·6	4¾	5185·0	+ ·6
III.	4*b*	1533·1	5261·5	5¼	5261·0	+ ·5
	2*b*	1443·5	5345·2	16	5345·0	+ ·2
IV (last).	5*d*	1217·8	5601·0	16	5601·0	0·0
	3*c*	1029·3	5856·3	19	5857·0	− ·7
V.	2*e*	894·9	6162·0	20¾	6161·0	+1·0
VI.	2*c*	720·1	6492·0	14¼	6493·0	−1·0
	2*b*	641·0	6721·2		6721·0	+ ·2

The series of intervals admits of the following contractions:

$$7\quad 38\quad 9\quad \underbrace{4\tfrac{3}{4}\quad 5\tfrac{1}{4}}\quad 16\quad 16\quad 19\quad \underbrace{20\tfrac{3}{4}\quad 14\tfrac{1}{4}}$$

$$\underbrace{7\quad 38}\quad 9\quad 10\quad 16\quad 16\quad \underbrace{19\quad 35}$$

$$\underbrace{45\quad 9}\quad 10\quad 16\quad 16\quad 54$$

$$\underbrace{54\quad 10}\quad 16\quad 16\quad 54$$

$$64\quad 16\quad 16\quad 54$$

r 8 times $\quad 8\quad 2\quad 2\quad 7$

Finally it must be noticed that this difference, $d=16$, according to this table may be taken an even number of times, up to , so as to give a physical interval; and $2d=32$ is 5 times 6·4, he mean of the intervals deduced for the several groups.

Thus we believe to have shown that the wave-lengths of Ditsheiner in regard to the regular distribution of the dark lines in he calcium spectrum fully confirm the laws enunciated in our reliminary investigation.

§ 19. *Barium spectrum.*—Having discussed the calcium specrum at considerable length, we may pass more lightly over the arium spectrum. As there are very few barium lines coincilent with those measured by Ditscheiner it may be sufficient to ultiply the difference between Kirchhoff's numbers with the angent or *grade* of the curve at the point considered (that is, he ratio between dW and dK) as taken from our drawings.

I	K	D	Grade.	D′		
2c	2031·1	41·6	·8	33·3	=	2×16·65
6c	1989·5					
1c	1287.5	13·3	1·25	16·6	=	1×16·6
8b	1274·2					
2a	1083·0	51·2	1·3	66·6	=	4×16·65
2a	1031·8	41·6	1·6	66·5	=	4×16·64
1b	890·2	15·9	1·9	30·2	=	2×15·1
4b	874·3					
3a	719·6	·9	2·5	2·3	=	$\frac{16\cdot1}{7}$
2	718·7					

On account of our method here used, only such lines cou be considered as are sufficiently close to admit of our reducti of the difference in K by simple multiplication with the grad All lines accessible give the interval 16·6—the mean of the si determinations is 16·3. It is also seen that the actual interva are very simple multiples hereof.

§ 20. *Magnesium spectrum.*

I	K	W	i	W′	E
6e	1655·6	5165·8	1	5166·07	—·27
6f	1648·8	5171·3		5171·3	·0
4c	1634·7	5181·0	2		
4g	1634·1	5181·5		5181·76	+·26

where $d = 5\cdot23$.

The line *6f* is Fraunhofer's *b*. The reduction was here effect arithmetically by means of the grade ·8; affecting only the la given number.

These three or four lines forming a natural group, the tot range or *3d*, i. e., 15·69, becomes characteristic of this element.

§ 21. *Strontium spectrum.* It will be noticed that this diffe ence is very nearly the same as the one previously found f barium and calcium. It thus becomes of great interest to asce tain whether or not strontium also herein agrees with the othe members of the group of alkaline earths. We have for stron tium

I	K	W	i	W′	E
3b	753·8	5400·0	54	5400·0	0·0
3a	1274·7	5532·0	3	5530·6	+1·4
4c	1320·6	5480·0	73	5482·3	—2·3
3	2857·9	4306·0		4307·0	—1·0
4a	2858·5				
2	2858·9				
	2859·4				

where $d = 16\cdot1$.

The intervals give

	54		3		73
or	18	:	1	:	$24\frac{1}{3}$

or the first to the last nearly as 3 : 4. Also 54+3=57 to 73 nearly as 7 : 9. The reductions were performed by means of the known grade, no one of the above strontium lines coinciding with Ditscheiner's lines.

§ 22. *The alkaline earths* give as the difference of wave-lengths characterizing their spectrum, magnesium 15·7, calcium 16·0, strontium 16·1, and barium 16·3, or in millimeters.

	Atomic weight.	Wave-difference. mm.
Magnesium, - - - -	12	0·00000157
Calcium, - - - - - -	20	0·00000160
Strontium, - - - - -	43·8	0·00000161
Barium, - - - - - -	68·5	0·00000163
	Mean value,	0·00000160

This coincidence is very remarkable, though the absolute identity can hardly be considered admissible. We shall come back to this point in our theoretical remarks.

§ 23. *Sodium spectrum.*—Professor Cooke of Cambridge has recently given[6] four drawings of the three sodium lines forming Fraunhofer's D. From these figures I find by measurement $D_1\alpha=12$ mm., $\alpha D_2=8$ mm., or the distances are as 3 : 2.

I am very sorry that I have not the determinations[7] of Wolf and Diacon in regard to the spectra of the alkalies. Interesting and valuable results might no doubt be derived from these observations.

§ 24. *Iron spectrum.*—We will now review the three large and beautiful groups of the iron spectrum as given in our first article.

Group I. $d_1=3{\cdot}29=0^{\text{mm}}{\cdot}000000329$.

	K	W	i	W′	E
4*b*	1200·6	5623·25	2	5621·58	+1·67
5*g*	1207·3	5615·00	4	5615·00	0·00
5*d*	1217·8	5602·00	5	5601·84	+ ·16
5*d*	1231·3	5585·65	4	5585·44	+ ·21
4*a*	1239·9	5572·65	1	5572·28	+ ·37
6*c*	1242·6	5569·40	1	5568·99	+ ·41
4*d*	1245·6	5565·65		5565·70	− ·05

The intervals are very simple. Only the first value of E may be considered important—may disappear upon the observation of a line between the first two given above.

The intervals give the following series:

$$\underbrace{2 \qquad 4} \qquad 5 \qquad \underbrace{4 \qquad 1} \qquad 1$$

or 1+5 5 5 1

showing that the following may be considered as intervals of higher order:

$$D_1'=4d_1=13{\cdot}16, \qquad D_1''=5d_1=16{\cdot}45$$

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[6] This Journal, 1866, xl, 179.

[7] This Journal, 1863, xxxv, 414.

GROUP II. $d_2=0{\cdot}9=0^{\mathrm{mm}}{\cdot}00000009$.

I	K	W	i	W′	E
4*d*	1337·0	5461·8	8	5461·3	+·5
6*c*	1343·5	5454·5	10	5454·1	+·4
5*d*	1351·1	5445·3	2	5445·1	+·2
5*b*	1352·7	5443·0	12	5443·3	−·3
5*b*	1362 9	5432·5	5	5432·5	0·0
6*d*	1367·0	5428·2	7	5428 0	+·2
5*b*	1372·6	5422·0	9	5421·7	+·3
4*c*	1380·5	5413·6	5	5413·6	0·0
4*c*	1384·7	5408·9	5	5409·1	−·2
6*c*	1389·4	5404·4	1	5404·6	−·2
5*d*	1390·9	5403·7	9	5403·7	0·0
5*c*	1397·5	5395·7	5	5395·6	+·1
4*c*	1401·6	5391·0	10	5391·1	−·1
4*c*	1410·5	5381·9	14	5382·1	−·2
6*c*	1421·5	5369·8	1	5369·5	+·3
5*b*	1423·0	5368·5	3	5368·6	−·1
5*b*	1425·4	5366·0	3	5365·9	+·1
5*b*	1428·2	5363·2		5363·2	0·0

The agreement is very close. The intervals are apparently not *very* simple, but they give

$$8\quad \underbrace{10\quad 2}_{12}\quad 12\quad \underbrace{5\quad 7}_{12}\quad 9\quad \underbrace{5\quad 5}_{10}\quad \underbrace{1\quad 9}_{10}\quad 5\quad 10\quad \underbrace{14\quad 1}_{15}\quad \underbrace{3\quad 3}_{6}$$

first multiple of 4 (viz., 8, 12, 12, 12) and in the latter half multiples of 5 (5, 5, 10, 5, 10, 15). But we may also group these as follows;

8 10 35 5 5 10 5 10 15 6

which are all, excepting extremes, multiples of 5; the extremes would by completion with adjacent parts not observed by Kirchhoff no doubt complete the series. Dividing by the common 5 we obtain the ratios

2 7 1 1 2 1 2 3

which again may be combined as

$$\underbrace{\qquad\qquad}_{9}\quad 1\quad \underbrace{\qquad\qquad}_{3}\quad \underbrace{\qquad\qquad}_{3}\quad 3$$

To the common factor 5 above corresponds an interval of $D_2=5d_2=4{\cdot}5$; to this further interval of 3 such corresponds an interval of $D_2{}'=3\times5d_2=15d_2=13{\cdot}5$. Both of these numbers have theoretical importance.

UP III. d_3=·709=0mm·0000000709.

I	K	W	i	W′	E
5*c*	2001·6	4921·5		4921·6	−·100
			4		
6*d*	2005·2	4918·8		4918·764	+·036
			2		
6*c*	2007·2	4917·3		4917·346	−·046
			38		
6*c*	2041·3	4890·4		4890·404	−·004
			1		
6*b*	2042·2	4889·8		4889·605	+·105
			19		
6*c*	2058·0	4876·4		4876·224	+·176
			9		
5*c*	2066·2	4870·0		4869·943	+·057
			1		
5*c*	2067·1	4869·2		4869·234	−·034
			16		
6*a*	2082·0	4857·7		4857·890	−·190

s good as nothing. The intervals arrange themselves as fol-
s:

$$4 \quad \underbrace{2 \quad 38}_{40} \quad \underbrace{1 \quad 19}_{20} \quad \underbrace{9 \quad 1}_{10} \quad \underset{16}{16}$$

4 : 2 : 1

)f course, the extremities not necessarily representing full
ervals, need not correspond; yet they are divisible by 4.
e natural intervals of a higher order in this group are: D_3'
$d_3=2·836$, $D_3''=5d_3=3·545$, $D_3'''=10d_3=7·090$, and per-
s $D_3^{iv}=9d_3=6·381$.

)omparing the different groups we see that the differences d
not the same, for

$$d_1=3·29, \qquad d_2=0·9, \qquad d_3=0·709,$$

we have for the intervals D

$$\left.\begin{aligned}\text{yellow, } D_1' &= 4d_1=13·16 \\ \text{green, } D_2' &=15d_2=13·5 \\ \text{blue, } 2D_3''' &=20d_3=14·18\end{aligned}\right\} \text{mean } 13·61,$$

ich are not quite equal, but good approximations. We shall
e back to this difference between the values—it seems that
values are a little larger in the blue than in the red.

n looking back upon the complicated groups of the rich iron
ctrum we cannot help being convinced that the dark lines are
tributed according to law, at *multiple* intervals as measured
the wave-length.

25. Before concluding this part of our investigation we will
npare our new results with those in our first article. In the
spectrum, the first group shows the following intervals:

then	8	12	17	11	3	4
now	2	4	5	4	1	1

ile the second groups gave

n	8	9	2	12	5	7	10	5	6	2	8	5	9	14	2	3	3
v	8	10	2	12	5	7	9	1	5	1	9	5	10	14	1	3	3

l the third group

then	4	2	38	1	18	9	1	17
now	4	2	38	1	19	9	1	16

By having more accurate data, the intervals become more simplified, especially in regard to the consolidating of groups of lines, as has been shown in the preceding paragraph.

In the synopsis of the calcium spectrum given on page 35 of our first article (this Jour., vol. xxxviii) the deviations amount to fully

$$a = 0^{mm} \cdot 0000030,$$

while in our present article the greatest deviation is only (§ 18)

$$a' = 0^{mm} \cdot 0000001$$

or $a : a' = 30 : 1$, only one-thirtieth of the previous one.

This comparison might be considerably enlarged—but the preceding is enough to show that far from disappearing by closer approximation and more accurate data, the laws there enunciated are becoming even more prominent and unmistakable in proportion as the data of observation have become more rigorous and reliable. It may therefore be hoped that continued investigation will tend to the better establishment of the laws referred to.

And it is in view of this greater confidence we are permitted to bestow on the regular distribution of the dark lines that we will venture upon the dangerous ground of a theoretical explanation of these wonderful facts and mysterious laws. The importance of spectral analysis in a practical point of view is established; its power and use in the laboratory is known and admired; its application to, or rather its creation of, *cosmical chemistry* is one of the greatest triumphs of physical science; but I believe that the day is not distant when spectral analysis will lay open to our view the chemical constitution of the *elements*, as I have already intimated in the concluding remarks of my first article on the dark lines. We will now try to give a little detail on this difficult and obscure, but most important point.

§ 26. *Origin of the dark lines.*—The mere aspect of the dark lines in any spectroscope conveys almost a moral conviction to the mind that they are allied in their origin to the dark lines of interference produced by thin plates and the like; for example, Talbot's interference phenomena produced by the interposition of a little piece of glass or mica between the eye and the ocular of a telescope directed to a spectrum, or Baden Powell's lines produced by a glass plate immersed in a prismatic trough filled with cassia-oil, etc.

This idea is already old, and I know of no definite refutation thereof. The lines produced by the vapors of iodine, bromine, hypochlorous acid and others, are closely allied to the regular spectral lines and the above lines of interference. Böttger[*]

[*] This Journal, 1863, vol. xxxv, p. 414.

states furthermore, that the *selenium* spectrum contains between the yellow and the violet *a very large number of equidistant dark lines.*

We have furthermore proved that though not *all* lines are present, corresponding to each successive equal interval, still all the known lines occupy positions in exact accordance with the equal intervals, that is, *the dark lines are as if from the whole number of exactly equidistant lines some had been blotted out or at least obscured by other causes.*

From all this, both the experimental and our theoretical evidence, we conclude, that *the dark lines are produced by interference.* We will now see whether we can account for some other peculiarities of their distribution by means of this hypothesis.

Erman, in his investigation of the absorption lines of iodine, bromine, etc., has shown that such lines produced by a plate of mica are the closer together, the thicker the plates, and that they are farther apart in the violet than in the red.

Considering matter to consist of solid atoms kept at certain distances from each other, we may either take the *distance* or the *dimension* of the atom as represented by *d*. But if *d* represented the distance, then the lines ought to change with the temperature, becoming more numerous with higher temperature—the dark lines would be as changing as the arrangement of the atoms. But we know the spectral lines to be permanent and immutable, as we suppose the atoms themselves to be.[*] We therefore must consider the dimensions of the atoms to correspond to *d*.

The atoms may be considered as having at the most three different dimensions, so that accordingly *the dark lines in the spectra of the elements are the result of the interference of at most three systems of interferences determined by the three dimensions of the atoms.*

We shall now compare some of the points involved herein to the results of observation laid down in the preceding paragraphs.

§ 27. *Is the difference constant* throughout the entire spectrum? or are the lines farther apart in the blue end of the spectrum, where the wave-length is smaller? The values of D obtained for the iron groups are: D in the yellow 13·16, in the green 13·5, in the blue 14·18; showing a slight increase in the same direction as demanded by Erman's formula. But the difference is too minute to admit of positive decision. Further and still more extended research is required. But we may safely affirm that in case any such variation is actually proved it will be found to be very small.

§ 28. *The lines of the same group are equidistant*—as required by this theory; for we have seen the same difference *d* to express all the numerous observed values for even very extensive groups of lines. Thus iron group I numbers 18 lines ranging through nearly 100 mm. of Kirchhoff's scale. As observed before, not all lines are actually observed—but that may be due to the coëxistence of the several systems caused by the several dimensions of the atoms, whereby also a variation in the intensity of the lines would be produced.

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[*] Mitscherlich's observations in regard to the spectrum of iodine admits of other explanation.

§ 29. *Influence of the dimension of the atoms.*—The greater d, the closer must be the dark lines. If this theoretical result be applied to the elements, considering their dimension as d, it would follow, if we adopt the hypothesis of one element, thereby measuring volume by the weight of the atom, that *the lines must be generally the closer the greater the atomic weight of the elements.*

In the following table A is the atomic weight taken from Will's Jahresbericht for 1863 as copied in the Smithsonian Report for 1864; d is the interval as found in the preceding paragraphs:

Metalloids.	A	d	Metals.	A	d
Hydrogen,	1	169·0	Magnesium,	12	5·23
Chlorine,	35·5	47·0	Calcium,	20	3·3–1·16
Bromine,	80·0	25·0	for several groups,		1·46–6·57
Iodine,	127·0	8·0			6·23
Nitrogen,	14·0	65·0	Iron,	28	$d_1=3{\cdot}29$
					$d_2={\cdot}9$
Oxygen,	8·0	$137\frac{1}{6}$			$d_3={\cdot}709$
			Mercury,	100	25·0

We notice first the great intervals of the metalloids as compared to those of the metals; also in general a greater interval for smaller atomic weight. This is the more conclusive for similar elements, as exemplified in the chlorine group.

We can not expect a more close agreement, for the dimensions and not the *volume*, proportional to the atomic weight, decide the distribution of the dark lines.

But the dimensions can only be found by for a moment accepting our hypothesis of the construction of the elements. We hope that the preceding will induce the reader to approach this question.

§ 30. *Dimensions of the atoms.*—We suppose all elementary atoms to be built up of the atoms of *one single matter*, the *urstoff*. Let the atomic weight of hydrogen referred to this prime-element be H—we have reason to believe that H=4.

Whatever be the form of these atoms, the laws of mechanics force them to arrange themselves regularly—and the most stable form will be the prism. If quite rectangular, and a, b, c be the number of primary atoms, in the three directions, we shall have (leaving out the factor H)

$$A = a.b.c.$$

If the atom has a quadratic base, $a=b$, we have

$$A = a^2 . c.$$

If provided with one or several pyramidal additions, we have

$$A = a.b.c + k.$$

Elements will of course show similar properties when of similar form (a theorem which I have demonstrated in my notes many years ago—and which is closely allied to the well known properties of isomorphous bodies). Thus prismatic atoms will, when they have the same base $a.b$ and only differ in length $c=n$, form a natural group

$$A = n \times ab,$$

or when quadratic, $A = n \times a^2$,

or when with pyramidal additions,

$$A = k + n.ab,$$

or $A = k + n.a^2$, etc.

We shall here refer only to those natural groups of elements that have been treated of in the preceding, viz: the chlorine group, the oxygen group, the group of the alkaline earths and the group of the alkalies.

Oxygen group; quadratic. Formula $A = n.4^2$.

	n	A	Calc.	Obs.	Error.
Oxygen,	1	$1\cdot4^2 =$	16	16	0·0
Sulphur,	2	$2\cdot4^2 =$	32	32	0·0
Selenium,	5	$5\cdot4^2 =$	80	80	0·0
Tellurium,	8	$8\cdot4^2 =$	128	128	0·0

Chlorine group; quadratic. Formula $A = n.3^2 \pm 1$.

	n	A	Calc.	Obs.	Error.
Fluorine,	2	$2\cdot3^2 + 1 =$	19	19	0·0
Chlorine,	4	$4\cdot3^2 - 1 =$	35	35·5	+·5
Bromine,	9	$9\cdot3^2 - 1 =$	80	80	0·0
Iodine,	14	$14\cdot3^2 + 1 =$	127	127	0·0

Alkaline group; quadratic with pyramid. $A = 7 + n.4^2$.

	n	A	Calc.	Obs.	Error.
Lithium,	0	7		7	0·0
Sodium,	1	$7 + 1.4^2 =$	23	23	0·0
Potassium,	2	$7 + 2.4^2 =$	39	39	0·0
Rubidium,	5	$7 + 5.4^2 =$	87	85·4	−1·6
Cæsium,	8	$7 + 8.4^2 =$	135	133·0	−2·0

Alkaline-earths group; quadratic. $A = n.2^2$.

	n	A	Calc.	Obs.	Error.
Magnesium,	3	$3\cdot2^2 =$	12	12	0·0
Calcium,	5	$5\cdot2^2 =$	20	20	0·0
Strontium,	11	$11\cdot2^2 =$	44	43·8	−·2
Barium,	17	$17\cdot2^2 =$	68	68·5	+·5

We cannot here go into any detail as to the relation of these formulæ to the numerical relations discovered by Carey Lea, Dumas and others; we hope soon to be enabled to publish our labors on the constitution of the elements. Neither can we here discuss these formulæ in the sense of the mechanics of atoms, deducing the physical and chemical properties of the elements from these formulæ; these interesting relations also we must delay till some future, but I hope not a very distant, time. Nor is this the place to discuss the few slight deviations noticed. Our aim here is to make use of these our old formulæ in spectral analysis.

First, then, we notice that the alkaline-earth metals are quadratic, so that their spectra are the result of two systems only of lines. Further, having a common base (2^2), they must show one set of differences, either absolutely or nearly equal—which, until we have an analytical investigation hereof, or fuller experimental results, we cannot decide. But this is precisely what has been found in §22, where the intervals were found respectively to be

Magnesium	0·00000157 mm.
Calcium......................	160
Strontium	161
Barium	163
Mean,	160

We might now discuss the occurrence of the *dimension-figures* of the atoms in the corresponding spectra as *intervals;* such for magnesium 2 and 3, for calcium 2 and 5, etc. Such coincidences are pretty numerous, but a fuller stock of still more reliable measurements will be required for the metallic spectra.

For the *chlorine group* we have the following dimension *n*, and distance *d* of lines, according to the given tables.

	n	*d*	*n.d*
Chlorine,	4	47	188
Bromine,	9	25	225
Iodine,	14	8	112

Then by doubling the interval 8 for iodine we get $14\times16=224$; or *the distance of the lines is nearly inversely proportional to the atomic dimension n.*

The other dimension of this group is 3, and is fairly represented in the intervals. Thus we have in the spectrum of *iodine*, neglecting the two extremes which are not necessarily complete, the intervals (see § 7)

$$21=3\times7$$
$$63=3\times21=3\times3\times7$$
$$23+4=27=3\times9\ =3\times3\times3$$

where the dimensions 3 of the base 3^2 is fully represented. The extreme intervals are $77=3\times26$ less 1, and $29=3\times10$ less 1.

Bromine gives the intervals (§ 6)

$$15=3\times5$$
$$1=1$$
$$3=3\times1$$

or again the dimension 3.

Chlorine has the intervals (§ 5)

$$9=3\times3$$
and $$5=3\times2-1$$

which, as 5 is probably not complete as far as it goes, again pronounces the dimension 3.

Thus we see in the spectra of the chlorine group a full and accurate representation of the atomic dimensions of these elements as expressed in their formula $A=n.3^2\pm1$, where $n=4$, 9, 14. I regret that I have no measurements for the *fluorine* spectrum.

§ 31. *Conclusion.*—In the preceding we have found, by means of the most accurate determinations of Ditscheiner and Plücker, that for the thirteen elements considered (viz., hydrogen, oxygen, nitrogen, chlorine, bromine, iodine, mercury, sodium, magnesium, calcium, strontium, barium, iron, and besides four compounds): *the dark lines of the elements are equidistant throughout the spectrum, but of varying intensity, many not being observed (or observable) at all; the intervals between the observable lines are expressible as simple multiples of the equal distance indicated by all.* It may be that the lines are a little farther apart in the more refracted blue part of the spectrum; see § 28.

We have, further, by considering the spectra of seven elements, viz., magnesium, calcium, strontium, barium and chlorine, bromine, iodine, found that *the dark lines of the elements are related to the atomic dimensions, considering the elements composed of one single primary element, "Urstoff."*

Thus we found the four alkaline-earth metals, having the same base, 2^2, to give almost identically the same principal distance of the lines; the mean was $0^{mm}\cdot00000160$ in wave-length. Also in the chlorine group most remarkable confirmations of this law were discovered.

It is now about twelve years since we first started the hypothesis of one primary matter as the element of elements, not in the shape of a philosophical idea, but as a physical hypothesis, *making it the basis of a theoretical, mechanical deduction of the properties of the elements.* Our first communications met with no favor: nevertheless we have continued to develop the consequences of this hypothesis.

It seems as if spectral analysis has shaken the axiom of the elementary nature of the so-called chemical elements in minds formerly adverse to questioning that axiom. Believing the scientific public now more apt to give a hearing to our theory, we intend to publish a series of articles, giving the properties of the chemical elements as functions of their atomic weight, this expressed as in the few instances given in § 30. We hope to prove, that *the unity of matter is as real as the unity of force*—both being the creative work of *one* all-pervading being.

Iowa City, Iowa, July, 1866.

ART. XLIX.—*Contribution to the Chemistry of the Mineral Springs of Onondaga, New York;* by CHARLES A. GOESSMANN, Ph.D., Chemist to the Salt Company of Onondaga.

[Concluded from page 218.]

III. *How does carbonate of lime act upon a solution of chlorid of magnesium?*—I boiled for sixteen hours two grams of finely pulverized carbonate of lime with from forty to fifty cubic centimeters of a solution of caustic magnesia, in hydrochloric acid, containing 1·336 grams of chlorid of magnesium; the latter solution had, for obvious reasons, been heated with a small excess of magnesia, and was consequently of slight alkaline reaction; the water which evaporated during boiling was from time to time renewed. The hot filtrate at the close of the treatment contained 0·2122 grams of lime; while the residue left upon the filter contained merely traces of magnesia. In repeating this experiment, but with the difference that I evaporated directly to dryness (at from 190° to 210° F.) and subsequently extracted the residue by means of 50 cubic centimeters of distilled water, I found that the solution thus resulting contained but 0·0794 grams of lime. The indifference of carbonate of lime to chlorid of magnesium at common temperatures, is a fact stated long ago by Karsten. The peculiar readiness with which the chlorid of magnesium parts, in a diluted solution and at a higher temperature, with some of its chlorine, in the form of hydrochloric acid, most likely causes in this instance the formation of some chlorid of calcium; while the freed magnesia enters into combination with the main bulk of its chlorid, forming a soluble oxychlorid. The duration of treatment and the temperature influence, to some extent, the degree of change, which in itself is very limited, and probably only applicable to higher temperatures.

IV. *How does carbonate of magnesia act upon chlorid of calcium?*—A solution of 0·7660 grams of chlorid of calcium in 50 cub. cent. of water was boiled for nearly an hour with an excess of carbonate of magnesia, equal to 1·0264 oxyd of magnesium when an examination proved that only 0·0246 grams of oxyd of calcium, equal to 0·0470 chlorid of calcium were left in the solution; while the missing lime had been replaced by magnesia: or, in other words, the chlorid of calcium, originally in solution, had been replaced by chlorid of magnesium. The excess of carbonate of magnesia employed for the operation, as left upon the filter, contained 0·6480 grams carbonate of lime (=0·7193 chlorid of calcium). The mutual decomposition is therefore, under proper circumstances, quite rapid and complete. There was no reason to apprehend any material difference in the results if chlorid of sodium should have been added. To prove this by experiment, some preliminary test of the solubility of carbonate of magnesia in a solution of chemically pure chlorid of sodium was required. I accordingly prepared such a solution of the strength of the Onondaga brines =16·5 per cent of chlorid of sodium, digested it for twenty-four hours with an excess of a well-washed commercial, as well as a freshly prepared, carbonate of magnesia, and obtained from the solution in both cases corresponding results—in the first case, 0·0297 per cent of magnesia, and in the second, 0·0265. Becoming thus informed of the solubility of carbonate of magnesia in a brine of the strength mentioned, I repeated the experiment, substituting Onondaga brine for a simple solution of chlorid of sodium. The brine already contained in solution (in the form of chlorid of magnesium) 0·0575 per cent of magnesia. An examination of the filtrate proved that the solution now contained 0·1324 per cent of magnesia. The whole amount of chlorid of calcium which the brine originally contained, and which was equal to 0·1400 per cent of chlorid of calcium, had been changed into a corresponding amount of chlorid of magnesium. Temperature and concentration do not, it seems, materially affect the action of carbonate of magnesia; while the solubility of the carbonate of lime formed is somewhat governed by both those conditions.

These few experiments are valuable in two directions. They explain to some extent, the reason for the preference here claimed among the various arrangements of the analytical results in the combinations presented below; and they indicate in what particular manner changes will take place when spring waters and brines become mixed. The mutual action of these two solutions upon each other in regard to changes in their chemical composition as pointed out, is, in my opinion, certain; and its magnitude is in a less degree a matter of time than of the quantities in which they happen to mix. As carbonic acid gas increases the solubility of a number of compounds here under consideration, its presence either as gas, or in the form of a bicarbonate, must, for that very reason, be highly favorable for bringing about some of the changes above described.

In the following final arrangement of my quantitative analytical results (see page 216), I favor No. I. in each case as the form best illustrative of the teachings of my investigations; for I am inclined to believe that the least objectionable basis for arranging analytical results, is to state them as they are obtained from an analysis of the solid residue left after a careful evaporation to dryness at common temperatures. In taking this view I am fully aware of the various changes of affinities which the alterations in temperature and concentration, in many instances, exert. Nos. II. and III, in each corresponding case, represent the same analysis in forms based upon views differing from those adopted in this paper.

Arrangement of the above various analytical results according to three different views.

I.

	a. (Willow st.)	*b.*(Prospect Hill.)	*c.*	*d.*	*e.*(Syracuse brine.)
Sulphate of lime,	0·3817	1·5214	0·6625	0·60250	5·7720
" magnesia,	0·1532	0·1764	....	....	....
" soda,	0·0311	0·0258	0·4508	0·22560	....
Carbonate of lime,	0·2946	0·2024	0·3942	0·26130	....
" prot. iron,	....	....	not det.	....	0·0440
Chlorid of sodium,	....	....	11·0844	10·02313	155·3170
" potassium,	....	....	....	....	0·1090
" magnesium,	0·0209	0·0169	0·3016	0·30340	1·4440
" calcium,	....	....	....	....	1·5330
Bromid of magnesium,	....	....	....	0·00270	0·0240
Silica,	0·0050	0·0035	0·0049	0·01770[a]	..
Free carbonic acid,	not det.	not det.	not det.	not det.	not det.
&c.				water	885·7570

II.

	a	*b.*	*c.*	*d.*
Sulphate of lime,	0·3817	1·5214	0·6625	0·60260
" magnesia,	0·1582	0·1977	0·3810	0·19065
Carbonate of lime,	0·2946	0·2024	0·3945	0·26130
Chlorid of sodium,	0·0257	0·0208	11·4572	10·20893
" magnesium,	....	....	....	0·15247
Bromid, "	....	....	....	0·00270
Silica,	0·0050	0·0045	0·0049	0·01770[a]
Free carbonic acid,	not det.	not det.	not det.[b]	not det.

III.

	a.	*b.*	*c.*	*d*
Sulphate of lime,	0·5851	1·7452	1·0943	0·81830
Carbonate of lime,	0·1451	0·0334	0·0769	0·10280
" magnesia,	0·1256	0·1384	0·2667	0·26820
Chlorid of sodium,	0·0257	0·0208	11·4572	10·39680
Bromid of magnesium,	....	....	....	0·00270
Silica,	0·0050	0·0045	0·0049	0·01770[a]
Free carbonic acid,	not det.	not det.	not det.[b]	not det.

[a] With some alumina. [b] Also some protox. iron and bromine undetermined.

The changes going on in this class of mineral waters as long as free carbonic acid or bicarbonates are present, as well as the consequences resulting from the application of higher temperatures for concentration, have, no doubt, been instrumental in causing views like those illustrated in No. 2 of each analysis. To represent the strongest acid in combination with the strongest base, as shown in No 3 of each analysis, is but slightly supported if the cases presented are subjected to a close investigation.

Glancing over the various analyses, *No.* 1 *in every instance*—disregarding for the present their differences in the relative proportions of the same compounds—we find it worthy of particular notice that the waters *a* and *b* contain chlorid of magnesium,[1] sulphate of soda, sulphate of magnesia and carbonate of lime; *c* and *d* contain the same compounds (except sulphate of magnesia) besides a considerable quantity of chlorid of sodium. The *brine proper* contains no sulphates except sulphate of lime, common to all; while of the chlorids, besides chlorid of sodium and chlorid of magnesium, *chlorid of calcium*, (instead of the carbonate of lime) is present. From what has been stated it appears that, so far as *a*, *b*, *c* and *d* are concerned, a more or less proportionate access of carbonate of magnesia, in proper form,[2] to a solution of chlorid of sodium and gypsum in the presence of carbonic acid, can explain their differences of composition. The brine, even if exposed to the influence of a proportionate access of carbonate of magnesia in a suitable condition, will ultimately change its composition in such a manner as to resemble most closely that of the different waters, provided the excess of chlorid of sodium is left out of consideration.

A short recapitulation of what has been adduced in these pages may demonstrate this last assertion. Carbonate of magnesia and gypsum form, in the presence of carbonic acid, sulphate of magnesia, and carbonate of lime. Carbonate of magnesia, gypsum and carbonic acid, in the presence of chlorid of sodium, arrange themselves, at a common temperature, according to their relative proportions;—in cases where the chlorid of sodium is either equivalent to, or exceeds the gypsum and the carbonate of magnesia—sulphate of soda, chlorid of magnesium and carbonate of lime will be produced, (Anthon), while under circumstances where the gypsum and carbonate of magnesia exceed the chlorid of sodium, or under the influence of a certain higher degree of temperature, the product will be sulphate of soda, chlorid of magnesium, carbonate of lime, and sulphate of magnesia. The *essential difference between the brine and the spring waters consists, as has been noticed, in the fact that the former contains chlorid of calcium instead of carbonate of lime contained in the latter.* The presence of chlorid of calcium in the brines practically excludes all sulphates, except sulphate of lime. A sufficient amount of carbonate of magnesia, added to the Onondaga brine, displaced quite readily the chlorid of calcium, by forming chlorid of magnesium and carbonate of lime; and would have displaced, finally, at the expense of the gypsum produced (if exceeding the chlorid of calcium in amount), sulphate of soda and chlorid of magnesium, provided an excess of free carbonic acid were secured during the whole action. Free carbonic acid never fails to be present in the cases presented. The changes which must occur when the brine and spring waters become mixed are, in view of the preceding statements, quite obvious. The sulphate of magnesia and sulphate of soda of the waters act upon the chlorid of calcium of the brine, producing sulphate of lime, and the chlorids of magnesium and sodium; while the carbonate of lime contained in the spring waters enter simply into the mixture. The observation, that in several instances carbonate of lime has been found covering the crystals of gypsum separated in the wooden vats during the concentration of the brine by solar heat, may find its proper explanation in the temporary existence of circumstances where an access of spring water to the brine has happened.

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[1] The brines of Onondaga contain traces of iodine; and so do the mineral waters, *c* and *d*, which are liable to a direct access of brine. I have omitted to place this fact more prominently on record, for the percentage is very small, though bromine exceeds the iodine; neither, however, have any immediate bearing on the various questions proposed. I have noticed that inferior pickle (mother-liquor), as sometimes found in the vats employed in the manufacture of salt by solar heat, shows frequently, during the summer season, decided indications of free iodine, and particularly of bromine. A peculiar condition of the atmosphere (ozone ?) seems to cause their disengagement. A similar reaction was noticed during a thunderstorm within the past summer season; a discharge of lightning passed from a telegraph pole (which it had struck) into a tank containing stored brine.

[2] Compare for further illustration, Dr. Perry's lectures, Chemical News, London, 1864, No. 218, etc. Also T. Sterry Hunt's publications before mentioned.

Looking at these facts in regard to the brines from a mere practical point of view, we must admit that an admixture of the waters of the surface springs is not likely to alter seriously the brines for any length of time; for its access, even on a small scale, will readily be observed in consequence of a rapid decrease in the strength of the brine. Economical considerations will, no doubt, alone suffice to urge, in time, efficient provisions to prevent such a serious depreciation of the commercial value of the brine. The salt manufactured from such a brine suffers no deterioration in properties, for the carbonate of lime imparted will be removed during the preliminary treatment of purification of the brine in the process of salt manufacture; and the additional amount of chlorid of magnesium, being in the cases contemplated mainly formed at the expense of chlorid of calcium, is thus of too trifling consequence to require any serious notice. In regard to the water of the springs, the result of a union with brine must prove quite different, if merely on account of the differences in concentration—a fact most unmistakably demonstrated by the analytical results obtained from the water of the springs *c* and *d*.

In adopting these views I have already entered, as may have been noticed, upon the discussion of the third question proposed on page 212. The specific properties of springs are due, by general consent, to the peculiar geological features and physical conditions of the locality where they originate, and which they traverse in their course to the surface. The brines and waters here in question will bear, no doubt, such marks. I am inclined to consider sample *a* a true representative of our surface formations, rich in partially disintegrated magnesian limestone, containing pebbles, etc. Samples *b*, *c* and *d* have undoubtedly suffered by contact with the shales of the Onondaga Salt Group, (gypseous). Samples *c* and *d* have been exposed, in addition, to an access of brine. The brine, most likely, originates at a lower depth, or a more remote point; its contact, if any, with a limestone formation containing magnesia, was either under disadvantageous circumstances or the formation was comparatively deficient in available carbonate of magnesia;[3] and its access to the waters *c* and *d*, may happen through fissures from below; or may be due to a surface percolation, or, in some instances, to both causes.

Proper, detailed records of the exact geological features of the localities here under consideration are quite deficient, as I have before mentioned; and examinations beyond the search for paying wells have not been made of late. Certainly no reliable records concerning the real conditions of the strata which directly underlie our area of red shale—if bearing salt water, etc., have come to my knowledge. The importance of such information, it must be admitted, cannot be overrated when engaged in tracing the origin of our brines. All that is at present known of the Onondaga brines and their sources may be summed up in the following statements:

1. The depressions in the Onondaga Shales are filled, in some localities, to an extent of nearly three to four hundred feet in depth with a diluvial deposit (detritus), varying from the coarsest gravel to the finest drift sand.[4]
2. The layers of coarse gravel and fine sand alternate without any distinct order or extent.
3. The gravel has frequently been formed into a conglomerate of great hardness, commonly called hard-pan—an impermeable layer which intersects, more or less efficiently, the various strata of the loose material.
4. A formation of more recent origin, consisting of a red loam

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[3] Dolomite and limestone of a dolomitic character—are slowly acted upon; Karsten, Haidinger, Hunt.

[4] The alternation of impermeable and loose strata may have some bearing upon the artesian character of the salt springs of Onondaga.

or loamy sand, covers, frequently, to a considerable extent, and to a depth of from 30 to 40 feet, the lower diluvial deposits.

5. The Onondaga red shale has been struck everywere, when the boring was continued beneath the brine-bearing drift masses; near the eastern embankment at from 90 to 180 feet;—toward the middle of the valley, between Salina and Geddes, at about 382 feet.

6. The brine proper makes its appearance at about 100 feet below the level of the surface.

7. The brine rises by means of boring and tubing to the level of the lake surface or within from 10 to 15 feet of it; the degree of its rise depends apparently on the specific-gravity of the brine. The most concentrated brine remains lowest.

8. The yield of a well, independently of the concentration of the brine, depends on the size of the gravel or sand around the lower termination of the tubing.

9. The brine does not increase or decrease in strength during the winter season, when no pumping takes place; its temperature is from 52° to 53° F.

10. The deep wells bear the heavy drafts of brine during the summer season without suffering in strength, while the shallow wells decline.[*]

11. The lowest depth does not, in all cases, guarantee the most concentrated brine.

12. The red shale apparently bears irregularities of stratification independent of the peculiar form of a basin.

13. The outcrop of the red shale on the eastern embankment of the lowlands—at Green Point—contains veins of gypsum interspersed with specular iron ore.

The occurrence of the peroxyd of iron as a pseudomorph of chlorid of iron in this local outcrop of the red shale is deserving of some attention. The peculiar manner in which the gypsum and that ore present themselves, sometimes in veins along side of each other, sometimes the latter surrounded and enclosed by the former, (besides the resemblance between some of the adjoining gypsum to the hardened masses of gypsum separated from its boiling solution in salt water,) are facts which seem to point to the existence of some peculiar local disturbance due to subterranean heat. The presence of serpentine at James street height, and the elevation of the localities where casts of chlorid of sodium have thus far been found, may also bear some relation to the presumed changes which some of the surrounding localities have suffered in regard to their original stratification.

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[*] The strength of the brine from the various wells varies from 45° to 76°, Salometer at 60°. The weaker brine is frequently found near the outskirts of the lake basin, (lowlands), and in the shallow wells. The strongest brine has thus far been obtained mainly from wells near the beach of the lake, and along the banks of Onondaga creek, (toward the center of the valley), between Geddes and Salina. The annual production of salt for the past ten years averages about seven million of bushels of 56 lbs.

To what immediate causes such local disturbance may be attributed, at what period it happened, to what extent, and with what consequences it may have manifested itself, must, for the present, remain a matter of conjecture. It is, however, gratifying to know that measures are under contemplation in proper quarters, which, it is to be hoped, may result in advancing our information relative to the highly important question: *What and where is the real source of the valuable brines of Onondaga?*

Syracuse, Feb., 1866.

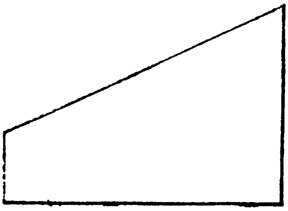
ART. L.—*On some new Manipulations;* by M. CAREY LEA.

I. GRADUATION OF BURETTES.

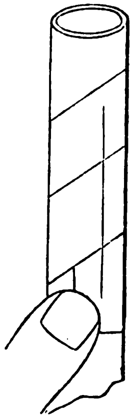
(1.) *Selection of Tube.*

ALTHOUGH no tube is sufficiently regular to dispense with subsequent exact calibration with a good balance, yet it is very desirable to have as near approach as possible to perfect uniformity, both in order to diminish the trouble of the weighings, and to a certain extent, that of the calculations, in actual use. In the following manner a variation in the diameter of the tube under examination, amounting to $\frac{1}{1200}$ or less can easily be detected.

1.



2.



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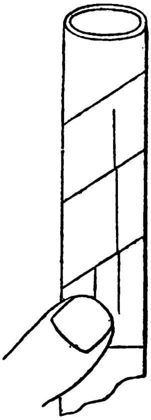


A piece of thin glazed letter paper is cut to the shape represented in fig. 1, this is wound tightly round the tube, making the successive folds perfectly correspond at bottom, a card is held to the paper, and a thin line is drawn with a fine pointed pencil, passing over three folds of the paper as represented in fig. 2.

The paper is then loosened, slipped a few inches along the tube and tightened again, keeping the lower edge exactly even. A very small difference in the diameter of the tube will cause the line to appear broken, as represented in fig. 3, instead of straight. A displacement from end to end amounting to one $\frac{1}{100}$ of an inch is easily observable, and as this difference corresponds to a little over six diameters, it is clear that a difference in the mean diameter of $\frac{1}{600}$ of an inch or less, makes itself evident to the unassisted eye. With an ordinary lens, a difference of $\frac{1}{200}$ of an inch is easily distinguishable, corresponding to a difference of diameter of $\frac{1}{200}$ of an inch. When great accuracy is desired it is advisable to stretch the paper band strongly beforehand, whereby it acquires a very slight permanent increase of length, and is not further extended by the very moderate force requisite to wrap it tightly round the tube.

It is so easy in this manner to detect slight differences that I have never been able to find a piece of tube of a foot or more in length which would bear this test when applied with the utmost rigor. But by examining large quantities of tube, pieces can be found sufficiently good for graduation. These are to be cut out from the tube in which they are found, and the part which has been ascertained to be sufficiently regular, must be carefully marked that the graduation may not be carried beyond that portion.

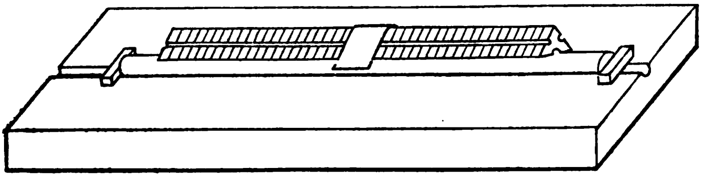
(2.) *Graduation of the Burette.*

All modes of graduation are necessarily the same in principle, and consist in transferring divisions from a scale to the tube. Accurate three-edged engineer's scales are easily to be had, and are very convenient for this purpose. The glass tube is secured by cleats into a groove in a block of wood. And here I may remark that the directions usually given in respect to the groove in which the tube rests, are certainly mistaken in recommending the groove to be part of a circle of a diameter *greater* than that of the tube. Such an arrangement causes the tube to rest loosely on the bottom of the groove, and to rock easily from side to side, touching as it does, in one place only. But if the groove is that of a circle of diameter rather *less* than that of the tube, the latter rests on the edges of the groove, and remains steadily in its position.

Before fastening the cleats, and while the tube is lying in the groove, touching its edges, a diamond pencil is drawn along the tube, the edge of the groove serving as a rule, and a line is made from one end of the space intended to be graduated to the other. The tube is then rotated on its axes a quarter of an inch, and another line is drawn parallel with the former. These lines serve to limit the length of the single degrees, each fifth and tenth being drawn beyond them.

The cleats are then fastened, the three-edged rule is laid with one of its edges resting on the tube, and its ends also are secured by cleats (these last are not represented in the figure). A piece of thin sheet brass (such as is used for enveloping combustion tubes) is bent at a sharp angle, and its right hand edges cut to an exact right angle with the bend. This is rested on the upper edge of the ruler, extending across its front face and over the tube—its right hand edge serves to guide the diamond pencil in transferring the divisions. Twentieths of an inch, or millimeters are easily and exactly ruled in this way, and the burette is ready for calibration.

4.



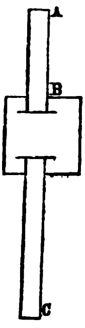
(3.) *Calibration.*

When a burette contains a portion of liquid, it is a matter of great nicety to determine the division or fraction of a division to which the surface of the liquid corresponds. To save the labor of holding the paper with the lower half blackened behind the tube, it is convenient to substitute card board, and to cut two parallel slits in it so as to form a band, which being slipped over the burette, the card maintains its own position. The use of paper blackened on the lower half, gives, according to Mohr, all the accuracy desirable. Bunsen, on the contrary, uses a cathetometer. That the former method is insufficient any one may satisfy himself by placing the card in position, and then moving his head slightly in a vertical direction when the black line which is assumed to mark the surface of the liquid, will be found to move also, and though the change is but small, it will be found that even the slightest movement of the eye produces a change in the position of the line which gives a difference in results easily detected by a good balance. Let us suppose that the observer notes the position of the black line before and after removing a portion of the liquid, he cannot be certain that his eye has occupied the same relative position in both cases, unless he takes due precautions. Every observer does not possess a cathetometer, and the following arrangement which is of extreme simplicity, will be found to answer every purpose.

A slip of wood is provided $\frac{1}{16}$ inch thick, 1½ inches wide, and 2 feet long. A piece of card about 4 inches square has two parallel slits made in it at one end, one slit half an inch from the top, the other the same distance from the bottom. These slits are of such a length as just to permit the piece of wood to be passed through them, presenting the appearance represented in the margin.

To make a reading, the half black card previously spoken of is slipped over the burette, and the line of separation between the white and black is brought one millimeter below the black line which marks the surface of the liquid. The instrument represented in the margin, is then placed beside the burette, the lower end resting on the table, and the card is raised or lowered until the edge B exactly corresponds with the line of separation in the black and white card. The stick is then drawn toward the observer, the end B still resting on the table, and the observer places his eye so as to keep the line B in range with the line of separation, and makes the reading.

5.



Another precaution which I regard as essential, and which I have nowhere seen mentioned, is the following: The observer should place himself where he has a strong *side light.* The half black half white card must not be placed parallel to the eyes of the observer, but must be turned toward the light, so as to make an angle of 45° with the line of vision. In this way a strong light is reflected from the card and thrown through the burette in a manner greatly conducive to clearness of vision, and the black line which marks the surface acquires a peculiar sharpness.

I cannot better illustrate the necessity of these precautions, (especially the use of the little instrument above described, and which may be termed the *eye adjuster*) than by the comparison of the following results obtained without them and with them.

Distilled water corresponding to the divisions of a tube prepared in the manner above described was carefully weighed in an accurate balance.

1. *Without the precautions.*

Mean of first three trials,	2·023 grams.
" " second " "	1·974 "

2. *With the precautions.*

Mean of first three trials,	2·046 grams.
" " second " "	2·047 "
" " two more "	2·050 ".

It will be seen that the trials without the precautions which I recommended gave results which were not only discordant with each other, but were *all wrong* and below the truth. With the precautions, on the contrary, the mean of the first three differed from that of the second three by only a single milligram, or less than the sixtieth part of a drop.

To show that the latter results were of no fortuitous exactitude, I may mention other instances. The distilled water corresponding with a given space was weighed *six* times and the mean found was 2·090 grams. Subsequently it was deemed advisable to submit this to a second verification, and the mean of *nine* trials gave 2·091. To the above two cases I may add the following:

Table No. 3,	mean of first 4 weighings,	2·054,	of 4 more,	2·0515
" " 4,	" " " 3 "	2·387,	" 4 "	2·362
" " 5,	" " " 3 "	2·365,	" 3 "	2·370
" " 6,	" " " 2 "	3·355,	" 2 "	3·551
" " 7,	" " " 2 "	3·577,	" 2 "	3·577
" " 8,	" " " 2 "	2·3605,	" 2 "	2·3630

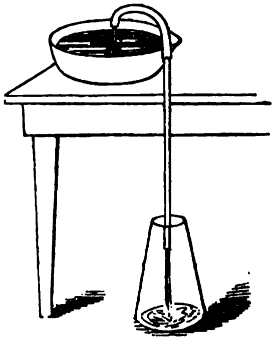
Comparing the first and second columns, that is, the first set of determinations, with the second we find that the maximum error was ·005, and the minimum ·0, or exact correspondence. The average of all the cases was 3·3 milligrams, or about one-twentieth of a drop. It would be difficult in burette analysis to obtain greater accuracy than this, for if we are using, for example, ten per cent solutions, the maximum error would reach half a milligram of the reagent used, and the average error would be one-third of a milligram.

I might greatly extend this table of verifications, but I think what I have cited will be sufficient to show that the very simple precautions which I here propose, viz., the position of the burette and card relatively to the light, and still more, the use of an eye-adjuster consisting of a slip of wood and sliding card are sufficient to carry us to the extreme limits of accuracy which the burette is capable of affording. It also shows that the burette is entitled to great confidence where carefully used and when the reactions which mark the termination of the operation are perfectly distinct and sharp.

II. INVERSE FILTRATION.

Large quantities of mixed solid and liquid matter may be filtered with a funnel of the smallest dimensions very speedily and conveniently, by proceeding in the following manner. A piece of stout muslin is strained over the mouth of a small funnel, and tied securely at the neck, taking care that the string covers all the folds perfectly. A piece of india-rubber tube is then passed over the open end of the stem of the funnel: this piece may either be several feet in length, or it may be shorter, and the difference be made up by inserting a glass tube. The funnel and tube are then filled with water, the open end of the tube is closed with the finger and the funnel is quickly inverted in the vessel containing the mixture to be filtered. The other end of the tube hangs down into a convenient vessel placed on the floor. If it is desirable that water shall not be added to the mixture, the funnel is inverted empty, and the air is drawn out by a pipette inserted into the open end of the india rubber tube.

6.



It is evident that this arrangement is a combination of filter and siphon, and the pressure of the column of water in the longer leg of the siphon expedites the operation very much and leaves the solid portions much drier than ordinary. As the liquid begins to be exhausted, the solid portions are to be gathered round the funnel with a spatula. When the filtrate ceases to run, the funnel is left full:—in order that this may not return upon the solid matter, the funnel is lifted out of the vessel and the broad end quickly turned uppermost, when the contents of the funnel flow down the tube.

When a precipitate is to be well washed this mode is evidently not applicable, although a tolerable washing is perfectly practicable. But when liquid and solid matters are to be quickly separated on a large scale, it is very useful. When masses of small crystals strongly retaining the mother water, are to be freed from it, this may be done more quickly and more thoroughly than by ordinary filtration. Many other cases will readily suggest themselves. For example, when potash has been boiled with lime to render it caustic in large vessels, it is usually drawn off with a siphon into bottles and left twelve to twenty-four hours to settle, and then must be carefully decanted. It is far less trouble to filter it in the above manner in the act of removing it by a siphon. And so with many of the rough operations which occasionally present themselves in the laboratory, and which are upon a scale rather exceeding the capacities of ordinary filtering funnels.

In the separation of crystals from the mother water, muslin will generally give a clear filtrate. In other cases it is necessary to place a piece of filtering paper inside the muslin. The paper must of course be of size sufficient to be secured by the twine at the neck of the funnel. But the force with which the column of water acts upon the muslin over the mouth of the funnel, draws it to a concave shape, and sometimes breaks the paper. To avoid this, the paper may be folded so as to fit the *inside* of the funnel, turning the edges over and securing them as before described. To do this requires a little dexterity; the diameter of the paper must be six or seven times that of the mouth of the funnel, it must be folded across in two rectangular directions in the ordinary way, then opened and reversed and from opposite points in the edges folded some distance past the middle. The funnel is then passed into it and the edges are passed round the neck of the funnel, the muslin is next placed over the paper and the whole is secured round the neck with cord.

ART. LI.—*Experiments on the Electro-motive Force and the Resistance of a Galvanic Circuit;* by HERMANN HAUG.

ENGAGED in investigations into the true numerical relations between the consumption of zinc in a galvanic battery, and the mechanical power which, by means of an electro-magnetic engine, may be derived from the galvanic current under varied conditions both in form and quantity, I began the work with studying the electro-motive force and the internal resistance of the battery, with the view of comparing the latter and the external resistances in conductors and helices. To determine the constants I used Ohm's method. A single galvanic cell was, by means of short, thick copper wires, connected with a tangent compass and a rheochord. The tangent compass was of Poggendorff's construction, the needle suspended by a hair, and modified according to Gaugain, thus securing the proportionality between the intensity and the tangent. The rheochord was of thin platinum wire, of Poggendorff's construction, modified by Dubois-Reymond. Both instruments were made by the best artists in Germany.

The law of the maximum of the effect of a galvanic battery requires both the internal and the external resistances to be the same. For many important reasons, which, however, have no direct connection with the object of this report, I thought it essential not to confine myself to the sequence of this law, but to vary the proportions between internal and external resistances of the electro-magnetic engine, as widely as circumstances would allow. I therefore considered it best to study the constants of the galvanic battery too, at once under similarly extreme conditions as regards the internal and external resistances. To do so I was further guided by circumstances and reflections which it may be well to state here with a few words.

From a careful study of the problem of practical application of the electro-magnetism as motive power, I have become convinced that it is merely a question of economy—economy in any material to be consumed, as well as economy in power to be derived from it, and to be transformed for the object in view. I found it necessary to exercise this economy from the very outset, and further on, at each and every step of all the processes and operations involved in the development and transformation of the power. One of these steps was the production of the galvanic current within a full circuit, from the free electricities of the poles of the open battery. A given quantity of electricity, in a galvanic current, will always, generally viewed, produce the same amount of electric process. But the free electricities requiring time for motion and combination (to use some kind of expression for the details of the electric process), remain during this time, subject to the influences of induction and the molecular qualities of the conductors. There may, consequently, occur something like a diffusion of the electricities, before the electric process is completed, and it is most probable that the direction of the electric process therefore, will not remain one and the same for the whole quantity of electricities, but that one part of the electric process will be executed in directions varying from the main direction, thus diminishing the actual effect of the process; and that the amount of this part, respectively the amount actually disposable, will depend upon and vary with certain circumstances. Now, as the electro-magnetic effect of the galvanic current is plainly and invariably governed by the direction of the electric process, it becomes evident that the amount of electro-magnetism to be derived from a given quantity of electricities, and available for any practical purpose, depends entirely on that part of the electric process which, at last, is going on in the main direction, and which is only a certain percentage of the whole electric process.

These considerations compelled me to be watchful, and this the more as I met with but one remark (by Buff, if I remember rightly) directly pertaining to this question. He found the electro-motive force of the battery increasing with the decrease of the measured intensities of the current. This circumstance, if true, would justify my views of the matter, and naturally must affect the economical applications of the galvanic current. The counter current too, de la Rive supposed to exist in a battery, may be mentioned in this connection.

My experiments gave results which it may be well to record. Though I cannot claim any excessive accuracy for my figures, still I am confident of their general correctness, and hope that they may be of some theoretical importance. They had an unfavorable influence on the proposed investigations.

The battery was a Bunsen's, one cell being of Stoehrer's, the other of Deleuil's construction; the former using a hollow carbon cylinder, the latter a plate of true gas coke. As liquids were used diluted sulphuric acid (respectively an acidulated solution of sulphate of zinc) and strong nitric acid, or a properly acidulated solution of bichromate of potash instead.

My method of experimenting was as follows. After preventing the conducting wires from affecting the tangent compass, and determining the intensity of the current within the shortest circuit possible, increasing length of platinum wire was, without opening the circuit, introduced into it. The reasons for not opening the circuit after each observation, were: 1, the time required for every observation after the circuit had been opened; 2, within the electro-magnetic machine, a similarly gradual change of resistance is effected, by means of the brake; 3, the rheochord is just designed, partly, to relieve of that tedious practice of opening the circuit every time a new resistance is introduced, and no objections have been raised, so far, against its proper use.

To calculate the constants of the battery I followed the common rule, combining the highest intensity within the shortest circuit, with each and every lower intensity. Let I_1 and I_2, be the intensities, E the electro-motive force, R the internal resistance, and P_2, P_3 the respective length of the platinum wire of the rheochord, and we have

$$R=\frac{P_2 I_2}{I_1-I_2}; \quad E=RI_1.$$

Table I gives the results of one series of observations with a Bunsen battery. For this table, as well as for all the others, I have to state that the figures on the "rheochord" line are centimeters of platinum wire, unless otherwise mentioned. In case two tangents are given, the observation of the direct intensities has been made, first in the beginning and afterwards at the end of the experiments; and every lower intensity has been combined with a highest intensity proportionately increased, or decreased, as the case might be, from the first to the last direct intensity. The rest of the table will explain itself, or will be explained later.

The figures for the internal resistance, as well as those for the electro-motive force, exhibit an increase amounting to 100 per cent within the range of the experiments, with the decrease of the lower intensity which is to be combined with the direct intensity, or with the increase of the external resistance. I confess I was not prepared to meet any such fact, since good experimenters frequently used pretty different and pretty high intensities; at any rate, the fact, if true, seemed to have been very much underrated. Of course, the lack of proper knowledge in this regard, and the general belief in the correctness of the law of Ohm, and its experimental proofs by many of the ablest observers, led at first to a severe doubt as to the value of my own observations compared with those of other experimenters. On the other hand, the increase was, on the whole, rather great, though it becomes very small from observation with 60 centimeters of platinum wire. Now the first circumstance, the general great increase, may perhaps be accounted for by the influence of temperature upon the resistances of conductors. The second circumstance, the slight increase from the experiment with 60 centimeters of platinum wire, may explain the fact of the results of other experimenters varying less, if they kept their experiments within narrower limits, and this was usually the actual case. The errors of observation may hide the normal increase, and may have led to the trifling with the matter, as generally observed.

Examining more closely some reports of other experimenters I was indeed able to trace the general increase of the constants of a galvanic battery with the decrease of the observed intensities. Thus I. Müller records the following results of observations with six cells of Daniell's construction. The resistance is expressed in meters of copper wire.

No. of battery.	Internal resistances for Meters of copper wire introduced into the circuit.					Mean values of them.
	5	10	40	70	100	
1.	2·85	2·85	3·20	....	....	2·97
2.	3·41	3·35	3·55	....	....	3·44
3.	3·02	3·05	3·23	....	....	3·10
4.	3·19	3·19	3·55	....	....	3·25
5.	3·08	3·13	3·40	....	....	3·21
6.	3·68	3·64	3·57	....	....	3·63
						20·6
Bat. of 6 cells,	18·20	19·03	18·01	....	....	18·31
				18·56	18·38	18·47

I think nobody can overlook the general increase of the internal resistance of every cell, except No. 6, with the increase of the external resistance, or with the decrease of the observed intensity. The sum of the mean resistances of all the six cells is 20·6, while the battery of all the six cells combined actually did not have more than 18·31, as calculated from the corresponding first three observations with 5, respectively 10, and 40 meters of copper wire in the circuit. Thus the internal resistance again results smaller, while the intensity of the observed currents is greater. Of course, the experimenter demonstrates from these figures that they are equal, to his satisfaction. But whatever the reason of this real increase of the galvanic constants may be, it seems to be a very wrong practice to accept the mean value of any number of observations as a true result for the internal resistance of a galvanic battery.

To convince myself more fully of the fact in question, I undertook other series of experiments, the results of which are given in tables II, III, IV, and V. Since, according to the formula $E=RI_1$, both the electro-motive force and the internal resistance maintain the same relation, I calculated for these, and all the following tables, only the internal resistance. Table III shows an increase from 2·98 to 5·52, not quite as much as table I; though the direct intensity is about the same, while the external resistance has been increased very much. Tables III and IV plainly demonstrate, as a general rule, that the increase of the internal resistance becomes less remarkable when the direct intensity of the battery is low. This rule, however, has its exceptions, and table V exhibits a very remarkable case. The direct intensity of this battery is less than in table II, still the increase of the internal resistance is much greater than in this table.

These results vexed me the more, since I undertook the experiments mainly for practical purposes. The experiments delayed my proposed investigation, and I could not anticipate any direct profit. I therefore concluded to give up, for the present, determining the constants of the battery, as they were not very constant; and to content myself with determining the resistances of the helices and conductors of the engine. It was with this object in view that I made the series of observations of table V, with 80 inches of thin copper wire in the circuit. Combining both the direct intensity (1·2712) and the intensity with 80 inches of copper wire in the circuit (·6964), with each and every of the other observations, I could calculate the resistance of the 80 inches of copper wire, expressed in centimeters of platinum wire. If C be the resistance of the copper wire, it follows from

$$I_1=\frac{E}{R},\quad I_2=\frac{E}{R+C},\quad I_3=\frac{E}{R+C+P}\quad \text{that}\quad C=\frac{P(I_1-I_2)I_3}{(I_2-I_3)I_1}.$$

As table V shows, the resistance of the copper wire as calculated in this manner, increases with the decrease of the observed intensities, just the same as the internal resistance did. And no limit being fixed, there could not be placed any reliability in any one of these calculated resistances, nor could I, with such a method, determine at all any resistance, under circumstances similar to those I proposed to try within the electro-magnetic machine. I now was compelled to investigate the matter at once.

As mentioned above, the reason for the increase of the internal resistance may be the relations existing between the intensity of the current, the heat developed by it within the circuit, and the influence of temperature upon the resistance of conductors. Indeed, the heating of the platinum wire makes our unit of resistance *greater* than the resistance of a unit of length of the wire at common temperature. And the liquids in the cup being heated, thereby offer *less* resistance. The internal resistance being diminished, and measured with an increased unit, will, of course, appear *smaller* than at low intensities. But knowing this, everybody ought to foresee the general increase of the constants of the galvanic current with the decrease of its intensity, instead of neglecting this fact entirely, or underrating its importance, and treating any variation of results simply as errors of observation.

I suppose that this interpretation, offering itself at the first glance, of the apparent increase of the constants, will be the one generally preferred. It is sustained by the circumstance, that the resistance of the platinum wire at beginning of red heat, is said to be a little over two times greater than its resistance at common temperature, and this ratio of increase of resistance coincides indeed pretty near with the maximum ratio of increase as given in the tables; intermediate cases not being comparable for want of proper control of the temperature of the platinum wire. Though I was pretty sure that in the observations of tables I and III, eight, respectively ten, centimeters of platinum wire had been rather far from becoming red hot, since the experiments were made at lamp light, and the wire deeply shaded; still, any lack in ratio of increase, from the heating of the platinum wire, might perhaps have been fully made up by the auxiliary of the liquids becoming warmer at high intensities, and their resistance being diminished. Being far from rejecting this explanation, in the whole, it is however just to remark, that it is indeed open to objection on a general ground, since all the experiments, the ratio of increase of the resistance of conductors with increase of temperature, as commonly understood, is based upon, must have been influenced, and their results augmented, by any other reason of such increase of resistance, if there be any other reason at all.

But this interpretation of the fact in question is liable to more direct and more conclusive objections, and I could not feel satisfied with it at all. In the first place, there were the experiments of table V, which seemed directly to contradict any such explanation. The ratio of increase of the internal resistance, the copper wire included, from 8·86 to 15·71 is = 1 : 1·773

The ratio of increase of resistance of the copper wire, calculated separately, from 4·0 to 7·1 is = 1 : 1·775

The true internal resistance, resulting either by subtracting the resistances of the copper wire from the respective whole internal resistances, or by direct calculation after the formula $R=\frac{PI_2I_3}{I_1(I_2-I_3)}$, increases from 4·86 to 8·61, or in the ratio of = 1 : 1·772

Thus the ratio of increase of resistance of the copper wire is the same as, or even a little greater than, that of the true internal resistance, or the liquids. Now it is evident, from the fact of the copper wire being heated much less than the platinum, and increasing its resistance in a higher degree than the latter (E. Becquerel gives the relations between resistance and temperature, for platinum =100+0·1861 t.° Cels., for copper =100+0·4097 t.° Celsius), that the ratio of increase of resistance of the copper wire should appear to be remarkably smaller than that of the liquids which constitute the main part of the internal resistance, under equal cicumstances. The explanation above referred to, so acceptable in case of the internal resistance of the liquids, seemed therefore to be a failure in case of the copper wire, since I presumed my experiments accurate enough at least to trace the difference, naturally to be expected, between the ratio of increase of resistance of liquids and that of copper.

But there is a point of far greater weight to be considered. The common rule for the calculation of the constants of a battery, by combining the highest direct intensity with some lower ones, or any other specific "method" certain experimenters prefer, is rather arbitrary, since Ohm's formula knows of no such restrictions, but permits to combine any two or more observations. And in thus combining any two observations I at first expected to get more reliable results, especially by throwing out those observations in which the platinum wire still had been heated very much. But these calculations seemed to run perfectly wild, giving much greater values than ever before. Combining, for instance, the intensity 0·115 with 100 centimeters of platinum wire, in table I, with each of the following three intensities, after the formula $R=\frac{P_2 I_2 - P_1 I_1}{I_1 - I_2}$, the values for the internal resistance became respectively 23·75, 31·67, 21·15, with a mean of 25·52, which, compared with the first value for the resistance (3·48), gave an increase from 1: 7·33, thus far greater than anybody could attempt to explain by way of influence of temperature. I now became pretty much convinced that there exists indeed some other reason for the increase of resistance with decrease of intensity, and a reason very much more powerful than the influence of temperature, and one able to conceal, by way of its great ratio and the errors of observations, comparatively slight differences in the ratio of increase of resistance of liquids and copper as proceeding from the influence of temperature.

On the other hand, those great and much varying values seemed to pronounce my observations as perfectly worthless. I could not imagine that facts of such important bearing could have been overlooked. Though, it is true, the experimental results the best observers have arrived at do not harmonize with each other to such a degree as to silence every doubt about their reliability.

However, my experiments were open to grave objections, as visibly indicated by the irregularity of the results. Indeed, the use of acids (nitric acid particularly) from former experiments may have caused some polarization to set in. The time required for a series of observations—the diminishing of the direct intensity usually occurring during this time—the change of the temperature and chemical composition of the liquids—the influence perhaps existing of the circuit being kept closed during the rapid but gradual change from one external resistance to another—all these circumstances may be considered susceptible of bringing on such extraordinary results as those just referred to.

TABLE I.

Bunsen's battery. Gas coke in nitric acid. The acids had been used previous to the experiments. The platinum wire, when shorter than 8 centimeters, became red hot.

Rheochord.	Compass.	Tangent.	Internal resist.	Electro-motive force.	Rheochord.	Compass.	Tangent.	Internal resist.	Electro-motive force.
0	60·8° 61·7	1·789 1·857	...	....	60	10·2°	·18	6·58	11·97
					80	8·	·14	6·66	12·16
8	28·5	·543	3·48	6·23	100	6·6	·115	6·69	12·27
10	26·2	·492	3·77	6·77	120	5·7	·099	6·84	12·6
20	19·25	·354	4·89	8·82	160	4·5	·079	7·11	13·15
40	13·0	·231	5·87	10·6	200	3·6	·063	6·97	13·05

TABLE II.

The same battery. The gas coke, after washing with water, had been exposed to the drying action of the air for 24 hours. The same acids.

Rheochord.	Compass.	Tangent.	Int. resist.	Rheochord.	Compass.	Tangent.	Int. resist.
0	55°	1·4281	....	100	5·1°	·0893	6·67
10	22·4	·4122	4·05	120	4·3	·0752	6·67
20	16	·2867	5·02	140	3·8	·0664	6·83
40	10·5	·1853	5·96	160	3·33	·0576	6·79
60	7·9	·1388	6·46	180	2·95	·0516	6·75
80	6·2	·1086	6·58	200	2·6	·0455	6·58

TABLE III.

The same battery, except the gas coke exchanged for another dry piece not used before. Acids the same.

Rheochord.	Compass.	Tangent.	Int. resist.	Rheochord.	Compass.	Tangent.	Int. resist.
0	61°	1·8040	....	180	2·8°	·0489	5·01
10	22·4	·4122	2·98	200	2·5	·0437	4·94
20	16	·2867	3·78	220	2·3	·0401	5·0
40	10·5	·1853	4·58	240	2·15	·0375	5·09
60	7·8	·1370	4·93	260	2·	·0349	5·13
80	6·15	·1077	5·08	280	1·9	·0332	5·25
100	4·95	·0867	5·05	300	1·85	·0323	5·47
120	4·25	·0744	5·16	320	1·8	·0314	5·67
140	3·65	·0638	5·13	340	1·65	·0288	5·52
160	3·2	·0559	5·12				

Table IV.—Bunsen's battery, with a common hollow cylinder. The acids had been used previously.

Rheochord.	Compass.	Tangent.	Int. resist.	Rheochord.	Compass.	Tangent.	Int. resist.
0	$\frac{15{\cdot}7^\circ}{15{\cdot}4}$	$\frac{{\cdot}2811}{{\cdot}2754}$	....	100	4·35	·0761	37·65
10	12·15	·2153	32·7	120	3·95	·0690	39·67
20	9·8	·1727	32·04	140	3·4	·0594	38·2
40	7·85	·1290	34·17	160	3·1	·0541	38·9
60	6·0	·1051	36·22	180	2·9	·0507	40·5
80	5·06	·0885	37·2	200	2·75	·0480	42·21

Table V.—Bunsen's battery, with gas coke. The acids had been used previously. The nitric acid, however, was mixed with sulphuric acid to restore the strength. The circuit contained 80 inches of thin copper wire.

Rheochord.	Compass.	Tangent.	Int. resist.	Resist. of 80 in. copper.	True inter. resist.	Rheochord.	Compass.	Tangent.	Int. resist.	Resist. of 80 in. copper.	True inter. resist.
0	$\frac{34{\cdot}5^\circ}{35{\cdot}2}$	·6964	....	....	....	40	9·7	·1709	13·01	5·88	7·13
2	29·6	·5681	8·86	4·0	4·86	50	8·5	·1495	13·67	6·18	7·49
3	25·2	·4706	8·34	3·77	4·57	60	7·5	·1317	13·99	6·33	7·66
6	22·3	·4101	8·59	3·88	4·71	70	6·6	·1157	13·95	6·31	7·64
8	20·3	·3699	9·06	4·10	4·96	80	6·1	·1068	14·49	6·55	7·94
10	18·8	·3405	9·57	4·32	5·25	90	5·6	·0980	14·74	6·66	8·08
12	17·4	·3134	9·82	4·44	5·38	100	5·2	·0910	15·03	6·79	8·24
14	16·4	·2943	10·24	4·63	5·61	120	4·5	·0787	15·29	6·91	8·38
16	15·5	·2773	10·59	4·79	5·80	140	3·8	·0664	14·75	6·67	8·08
18	14·7	·2623	10·88	4·92	5·96	160	3·5	·0612	15·41	6·97	8·44
20	14·	·2493	11·15	5·04	6·11	180	3·2	·0559	15·71	7·10	8·61
30	11·4	·2016	12·22	5·53	6·69	200	2·9	·0507	15·71	7·10	8·61

The 80 inches of copper wire excluded, and the circuit closed directly, the current gave $\frac{51{\cdot}2^\circ}{52{\cdot}4}$ at the compass, corresponding to a mean tangent of 1·2712.

(To be continued.)

Art. LII.—*On the Spectrum of a new Star in Corona Borealis;*[1] by William Huggins, F.R.S., and W. A. Miller, M.D., Treas. R.S.[2]

Yesterday, May the 16th, one of us received a note from Mr. John Birmingham of Tuam, stating that he had observed on the night of May 12 a new star in the constellation of Corona

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[1] The Astronomer Royal wrote to one of us on the 18th, "Last night we got a meridian observation of it; on a rough reduction its elements are—

R. A. 1866, May 17, - - - - $15^h\,53^m\,56^s{\cdot}08$
N. P. D., - - - - - - 63° 41′ 53″

agreeing precisely with Argelander, No. 2765 of 'Bonner Sternverzeichniss,' declination +26°, magnitude 9·5." Mr. Baxendell writes on the 21st, "It is probable that this star will turn out to be a variable of long or irregular period, and it may be conveniently at once designated *T* Coronæ." Sir John Herschel informs one of us that on June 9, 1842, he saw a star of the sixth magnitude in Corona very nearly in the place of this strange star. As Sir John Herschel's position was laid down merely by naked eye allineations, the star seen by him may have been possibly a former temporary outburst of light in this remarkable object.

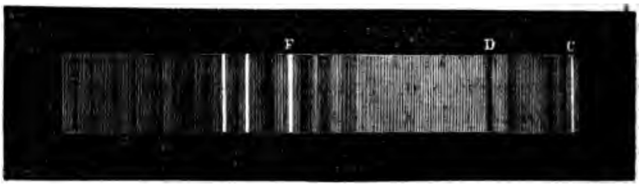
[2] From the Proceedings of the Royal Society, No. 84, 1866.

Borealis. He describes the star as "very brilliant, of about the 2d magnitude." Also Mr. Baxendell of Manchester wrote to one of us, giving the observations which follow of the new star, as seen by him on the night of the 15th instant:

"A new star has suddenly burst forth in Corona. It is somewhat less than a degree distant from ε of that constellation in a southeasterly direction, and last night was fully equal in brilliancy to β Serpentis or γ Herculis, both stars of about the 3d magnitude."

Last night, May 16th, we observed this remarkable object. The star appeared to us considerably below the 3d magnitude, but brighter than ε Coronæ. In the telescope it was surrounded with a faint nebulous haze, extending to a considerable distance, and gradually fading away at the boundary.[3] A comparative examination of neighboring stars showed that this nebulosity really existed about the star. When the spectroscope was placed on the telescope, the light of this new star formed a spectrum unlike that of any celestial body which we have hitherto examined. The light of the star is compound, and has emanated from two different sources. Each light forms its own spectrum. In the instrument these spectra appear superposed. The principal spectrum is analogous to that of the sun, and is evidently formed by the light of an incandescent solid or liquid photosphere, which has suffered absorption by the vapors of an envelope cooler than itself. The second spectrum consists of a few bright lines, which indicate that the light by which it is formed was emitted by matter in the state of luminous gas.[4] These spectra are represented with considerable approximative accuracy in the annexed diagram.

Spectrum of absorption and spectrum of bright lines forming the compound spectrum of a new star near ε Coronæ Borealis.



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[3] On the 17th this nebulosity was suspected only; on the 19th and 21st it was not seen.

[4] The position of the groups of dark lines shows that the light of the photosphere, after passing through the absorbent atmosphere, is yellow. The light, however, of the green and blue bright lines makes up to some extent for the green and blue rays (of other refrangibilities) which have been stopped by absorption. To the eye, therefore, the star appears nearly white. However, as the star flickers, there may be noticed an occasional preponderance of yellow or blue. Mr. Baxendell, without knowing the results of prismatic analysis, describes the impression he received to be "as if the yellow of the star were seen through an overlying film of a blue tint."

Description of the spectrum of absorption.—In the red a little more refrangible than Fraunhofer's C are two strong dark lines. The interval between these and a line a little less refrangible than D is shaded by a number of fine lines very near each other. A less strongly marked line is seen about the place of solar D. Between D and a portion of the spectrum about the place of *b* of the solar spectrum, the lines of absorption are numerous, but very thin and faint. A little beyond *b* commences a series of close groups of strong lines; these follow each other at small intervals, as far as the spectrum can be traced.

Description of the gaseous spectrum.—A bright line, much more brilliant than the part of the continuous spectrum upon which it falls, occupies a position which several measures make to be coincident with Fraunhofer's F.[*] At rather more than one-fourth of the distance which separates F and G, a second and less brilliant line was seen. Both these lines were narrow and sharply defined. Beyond these lines, and at a distance a little more than one-third of that which separates the second bright line from the strongest bright one, a third bright line was observed. The appearance of this line suggested that it was either double or undefined at the edges. In the more refrangible part of the spectrum, probably not far from G of the solar spectrum, glimpses were obtained of a fourth and a faint bright line. At the extreme end of the visible part of the less refrangible end of the spectrum, about C, appeared a line brighter than the normal relative brilliancy of this part of the spectrum. The brightness of this line, however, was not nearly so marked in proportion to that of the part of the spectrum where it occurs, as was that of the lines in the green and blue.[†]

General conclusions.—It is difficult to imagine the present physical constitution of this remarkable object. There must be a photosphere of matter in the solid or liquid state emitting light of all refrangibilities. Surrounding this must exist also an atmosphere of cooler vapors, which give rise by absorption to the groups of dark lines.

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[*] On the 17th, the lines of hydrogen, produced by taking the induction-spark through the vapor of water, were compared in the instrument simultaneously with the bright lines of the star. The brightest line coincided with the middle of the expanded line of hydrogen which corresponds to Fraunhofer's F. On account of the faintness of the red end of the spectrum, when the amount of dispersion necessary for these observations was employed, the exact coincidence of the line in this part of the spectrum with the red line of hydrogen, though extremely probable, was not determined with equal certainty.

[†] The spectra of the star were observed again on the 17th, the 19th, the 21st, and the 23d. On these evenings no important alteration had taken place. On the 17th and succeeding evenings, though the spectrum of the waning star was fainter than on the 16th, the red bright line appeared a little brighter relatively to the green and blue bright lines. On the 19th and 21st the absorption lines about *b* were stronger than on the 16th. From the 16th the continuous spectrum diminished in brightness more rapidly than the gaseous spectrum, so than on the 23d, though the spectrum as a whole was faint, the bright lines were brilliant when compared with the continuous spectrum.

Besides this constitution, which it possesses in common with the sun and the stars, there must exist the source of the gaseous spectrum. That this is not produced by the faint nebulosity seen about the star is evident by the brightness of the lines, and the circumstance that they do not extend in the instrument beyond the boundaries of the continuous spectrum. The gaseous mass from which this light emanates must be at a much higher temperature than the photosphere of the star; otherwise it would appear impossible to explain the great brilliancy of the lines compared with the corresponding parts of the continuous spectrum of the photosphere. The position of two of the bright lines suggests that this gas may consist chiefly of hydrogen.

If, however, hydrogen be really the source of some of the bright lines, the conditions under which the gas emits the light must be different from those to which it has been submitted in terrestrial observations; for it is well known that the line of hydrogen in the green is always fainter and more expanded than the brilliant red line which characterizes the spectrum of this gas. On the other hand, the strong absorption indicated by the line F of the solar spectrum, and the still stronger corresponding lines in some stars, would indicate that under suitable conditions hydrogen may emit a strong luminous radiation of this refrangibility.[7]

The character of the spectrum of this star, taken together with its sudden outburst in brilliancy and its rapid decline in brightness, suggests to us the rather bold speculation that, in consequence of some vast convulsion taking place in this object, large quantities of gas have been evolved from it; that the hydrogen present is burning by combination with some other element and furnishes the light represented by the bright lines; also that the flaming gas has heated to vivid incandescence the solid matter of the photosphere. As the hydrogen becomes exhausted, all the phenomena diminish in intensity, and the star rapidly wanes.

In connexion with this star, the observations which we made upon the spectra of α Orionis and β Pegasi, that they contain no absorption lines of hydrogen, appear to have some new interest. The spectra of these stars agree in their general characters with the absorption spectrum of the new star. The whole class of white stars are distinguished by having hydrogen lines of extraordinary force. It may also be mentioned here that we have found that the spectra of several of the more remarkable of the variable stars, namely, those distinguished by an orange or ruddy tint, possess a close general accordance with those of α Orionis, β Pegasi, and the absorption spectrum of the remarkable object described in this paper. The purely speculative idea presents itself from these observations, that hydrogen probably plays an important part in the differences of physical constitution which apparently separate the stars into groups, and possibly also in the changes by which these differences may be brought about.*

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[7] On the dependence of the relative characters of the bright lines of hydrogen upon conditions of pressure and temperature, see Plücker and Hittorf, Phil. Trans., *1865, p.* 21.

ART. LIII.—*On the Source of Muscular Power*; by EDWARD FRANKLAND, Ph.D., F.R.S.[1]

WHAT is the source of muscular power? Twenty years ago, if this question had been asked, there were but few philosophers who would have hesitated to reply, "The source of muscular power is that peculiar force which is developed by living animals, and which we term the *vital force!*" but the progress of scientific discovery has rendered the view implied in such an answer so utterly untenable that, at the present moment, no one possessing any knowledge of physical science would venture to return such a reply. We now know that an animal, however high its organization may be, can no more *generate* an amount of force capable of moving a grain of sand, than a stone can fall upwards or a locomotive drive a train without fuel. All that such an animal can do is to liberate that store of force, or *potential energy*, which is locked up in its food. It is the *chemical change* which food suffers in the body of an animal that liberates the previously pent-up forces of that food, which now make their appearance in the form of *actual energy*—as heat and mechanical motion.

From food, and food alone, comes the *matter* of which the animal body is built up; and from food alone come all the different kinds of *physical force* which an animal is capable of manifesting.

The two chief forms of force thus manifested are *Heat* and *Muscular motion* or *mechanical work*, and these have been almost universally traced to two distinct sources—the *heat* to the oxyd-

* Mr. Baxendell sends us the following table of magnitudes:

May	15	at	12^h	0^m	G.M.T.,	T Coronæ	=3·6 or 3·7 magnitude.
"	16	"	10	30	"	"	4·2
"	17	"	11	0	"	"	4·9
"	18	"	12	30	"	"	5·3
"	19	"	12	15	"	"	5·7
"	20	"	12	30	"	"	6·2
"	21	"	12	0	"	"	7·3
"	22	"	11	15	"	"	7·7
"	23	"	10	30	"	"	7·9
"	24	"	10	30	"	"	8·1

[1] From the Proc. Roy. Inst. of Great Britain, June 8, 1866.

ation of the *food*, and the *mechanical work* to the oxydation of the *muscles.*

This doctrine, first promulgated, the speaker believed, by Liebig, occupies a prominent position in that philosopher's justly celebrated 'Chemico-Physiological Essays.'

In his work entitled 'Die organische Chemie in ihrer Anwendung auf Physiologie und Pathologie, Braunschweig, 1842,' Liebig says, "All experience teaches that there is only one source of mechanical power in the organism, and this source is the transformation of the living parts of the body into lifeless compounds. . . . This transformation occurs in consequence of the combination of oxygen with the substance of the living parts of the body." And again, in his 'Letters on Chemistry, 1851,' p. 366, referring to these living parts of the body, he says, "All these organized tissues, all the parts which in any way manifest force in the body are derived from the albumen of the blood; all the albumen of the blood is derived from the plastic or sanguineous constituents of the food, whether animal or vegetable. It is clear, therefore, that the plastic constituents of food, the ultimate source of which is the vegetable kingdom, are the conditions essential to all production or manifestation of force, to all these effects which the animal organism produces by means of its organs of sense, thought, and motion." And again, at page 374, he says, "The sulphurized and nitrogenous constituents of food determine the continuance of the manifestations of force; the non-nitrogenous serve to produce heat. The former are the builders of organs and organized structures, and the producers of force; the latter support the respiratory process, they are *materials for respiration.*"

This doctrine has since been treated as an almost self-evident truth in most physiological text-books; it has been quite recently supported by Ranke;[1] and, in his lecture 'On the Food of Man in relation to his Useful Work, 1865,' Playfair says, page 37, "From the considerations which have preceded, we consider Liebig amply justified in viewing the non-nitrogenous portions of food as mere heat-givers. . . . While we have been led to the conclusion that the transformation of the tissues is the source of dynamical power in the animal." At page 30 he also says, "I agree with Draper and others in considering the contraction of a muscle due to a disintegration of its particles, and its relaxation to their restoration. . . . All these facts prove that transformation of the muscle through the agency of oxygen is the condition of muscular action." Finally, in a masterly review of the present relations of chemistry to animal life, published in March last,[2] Odling says, page 98, "Seeing, then, that muscular exertion is really dependent upon muscular oxydation, we have to consider what should be the products, and what the value of this oxydation." . . . And again, page 103, "The slow oxydation of so much carbon and hydrogen in the human body, therefore, will always produce its due amount of heat, or an equivalent in some other form of energy; for while the latent force liberated by the combustion of the carbon and hydrogen of fat is expressed *solely in the form of heat*, the combustion of an equal quantity of the carbon and hydrogen of voluntary muscle is expressed *chiefly in the form of motion*."

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[1] 'Tetanus eine Physiologische Studie.' Leipzig, 1865.

[2] 'Lectures on Animal Chemistry.'

Nevertheless, this view of the origin of muscular power has not escaped challenge. Immediately after its first promulgation, Dr. J. R. Mayer wrote,[4] "A muscle is only an apparatus by means of which the transformation of force is effected, *but it is not the material by the chemical change of which* mechanical work is produced." He showed that the 15 lbs. of dry muscles of a man weighing 150 lbs. would, if their mechanical work were due to their chemical change, be completely oxydized in eighty days, the heart itself in eight days, and the ventricles of the heart in two and a half days. After endeavoring to prove by physiological arguments that not one per cent of the oxygen absorbed in the lungs could possibly come into contact with the substance of the muscles, Mayer says, "The fire-place in which this combustion goes on is the interior of the blood vessels, the *blood* however—a slowly-burning liquid—is the oil in the flame of life. . . . Just as a plant-leaf transforms a given mechanical effect, *light*, into another force, *chemical difference*, so does the muscle produce mechanical work at the cost of the chemical difference consumed in its capillaries. Heat can neither replace the sun's rays for the plant, nor the chemical process in the animal: every act of motion in an animal is attended by the consumption of oxygen and the production of carbonic acid and water; every muscle to which atmospheric oxygen does not gain access ceases to perform its functions."

But Mayer was not the first to conceive this view of muscular action. Nearly 200 years ago, a Bath physician, Dr. John Mayow,[5] distinctly stated that for the production of muscular motion two things are necessary—the conveyance of combustible substances to the muscle by the blood, and the access of oxygen by respiration. He concluded that the chief combustible substance so used was fat. A century before Priestley isolated oxygen, Mayow was aware of its existence in the air, in nitre, and in nitric acid; he knew that combustion is supported by the oxygen of the air, and that this gas is absorbed in the lungs by the blood, and is absolutely necessary for muscular activity.

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[4] 'Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel,' 1845.

[5] 'De Motu musculari,' 1681. Mayow was born in 1645, and died 1679.

For two decades this doctrine sank into oblivion; and it is only within the last two years that it has been again advanced, chiefly by Haidenhain,[6] Traube, and, to a limited extent, by Donders.[7]

Experimental evidence was, however, still wanting to give permanent vitality to the resuscitated doctrine; for although the laborious and remarkable investigations of Voit[8] and of Edward Smith[9] point unmistakably in the direction of Mayow and Mayer's hypothesis, yet the results of these physiologists were not sufficiently conclusive to render the opposite view untenable. This want of data of a sufficiently conclusive character has been supplied by a happily conceived experiment undertaken by Fick and Wislicenus in the autumn of last year, and described in the 'Philosophical Magazine,' vol. xxxi, p. 485. In the application of these data, however, to the problem now under consideration, one important link was found to be wanting, viz., the amount of actual energy generated by the oxydation of a given weight of muscle in the human body. Fick and Wislicenus refer to this missing link in the following words: "The question now arises what quantity of heat is generated when muscle is burnt to the products in which its constituent elements leave the human body through the lungs and kidneys? At present, unfortunately, there are not the experimental data required to give an accurate answer to this important question, for neither the heat of combustion of muscle nor of the nitrogenous *residue* (urea) of muscle is known." Owing to the want of these data, the numerical results of the experiments of Fick and Wislicenus are rendered less conclusive against the hypothesis of muscle combustion than they otherwise would have been, while similar determinations, which have been made by Edward Smith, Haughton, Playfair, and others, are even liable to a total misinterpretation from the same cause.

The speaker stated that he had supplied this want by the calorimetrical determination of the actual energy evolved by the combustion of muscle and of urea in oxygen. Availing himself of these data he then proceeded to the consideration of the problem to be solved, the present condition of which might be thus summed up:—It is agreed on all hands that muscular power is derived exclusively from the mutual chemical action of the food and atmospheric oxygen; but opinions differ as to whether that food must first be converted into the actual organized substance of the muscle, before its oxydation can give rise to mechanical force, or whether it is not also possible that muscular work may be derived from the oxydation of the food, which has only arrived at the condition of blood and not of organized muscular tissue.

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[6] 'Mechanische Leistung Wärmeentwickelung und Stoffumsatz bei der Muskelthätigkeit,' 1864.

[7] As this is passing through the press, the speaker has become aware that Messrs. Lawes and Gilbert advocated this doctrine in 1852, and repeatedly since; their opinions being founded upon experiments on the feeding of cattle.

[8] 'Untersuchungen über den Einfluss des Kochsalzes, des Kaffeés und der Muskelbewegungen auf den Stoffwechsel,' p. 150. Munich, 1860.

[9] Phil. Trans., 1861, p. 747.

The importance of this problem can scarcely be overrated; it is a corner-stone of the physiological edifice, and the key to the phenomena of the nutrition of animals. For its satisfactory solution the following data require to be determined:

1st. The amount of force or actual energy generated by the oxydation of a given amount of muscle in the body.

2d. The amount of mechanical force exerted by the muscles of the body during a given time.

3d. The quantity of muscle oxydized in the body during the same time.

If the total amount of force involved in muscular action, as measured by the mechanical work performed, be greater than that which could possibly be generated by the quantity of muscle oxydized during the same time, it necessarily follows that the power of the muscles is not derived *exclusively* from the oxydation of their own substance.

As regards the first datum to be determined, it is necessary to agree upon some unit for the measurement of mechanical force. The unit most commonly adopted is that represented by the lifting of a kilogram weight to the height of one meter. The researches of Joule and Mayer have connected this standard unit with heat;—they prove that the force required to elevate this weight 425 times will, when converted into heat, raise the temperature of an equal weight of water 1° C. If this weight were let fall from a height of 425 meters, its collision with the earth would produce an amount of heat sufficient to raise the temperature of 1 kilogram of water 1° C. The same heating effect would also of course be produced by the fall of 425 kilograms through 1 meter. This standard of force is termed a *meterkilogram;*[16] and 425 meterkilograms are equal to that amount of heat which is necessary to raise the temperature of 1 kilogram of water through 1° C. If then it be found that the heat evolved by the combustion of a certain weight of charcoal or muscle, for instance, raises the temperature of 1 kilogram of water through 1° C., this means, when translated into mechanical power, 425 meterkilograms. Again, if a man weighing 64 kilograms climbs to a height of 1,000 meters, the ascent of his body to this height represents 64,000 meterkilograms of work; that is, the labor necessary to raise a kilogram weight to the height of 1 meter 64,000 times.

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[16] I follow the example of the Registrar-General in abbreviating the French word *gramme* to gram.

In order to estimate the amount of actual energy generated by the oxydation of a given amount of muscle in the body, it is necessary to determine, first, the amount of actual energy generated by the combustion of that amount of muscle in oxygen, and then to deduct from the number thus obtained the amount of energy still remaining in the products of the oxydation of this quantity of muscle which leave the body. Of these products, urea and uric and hippuric acids are the only ones in appreciable quantity which still retain potential energy on leaving the body, and of these the two latter are excreted in such small proportions that they may be considered as urea without introducing any material error into the results.

These determinations were made in Lewis Thompson's calorimeter, which consists of a copper tube to contain a mixture of chlorate of potash with the combustible substance, and which can be enclosed in a kind of diving-bell, also of copper, and so lowered to the bottom of a suitable vessel containing a known quantity (2 liters) of water. The determinations were made with this instrument in the following manner:—19·5 grams of chlorate of potash, to which about one-eighth of peroxyd of manganese was added, was intimately mixed with a known weight (generally about 2 grams) of the substance whose potential energy was to be determined, and the mixture being then placed in the copper tube above mentioned, a small piece of cotton thread, previously steeped in chlorate of potash and dried, was inserted in the mixture. The temperature of the water in the calorimeter was now ascertained by a delicate thermometer; and the end of the cotton thread being ignited, the tube with its contents was placed in the copper bell and lowered to the bottom of the water. As soon as the combustion reached the mixture a stream of gases issued from numerous small openings at the lower edge of the bell and rose to the surface of the water—a height of about 10 inches.

At the termination of the deflagration, the water was allowed free access to the interior of the bell, by opening a stop-cock connected with the bell by a small tube rising above the surface of the water in the calorimeter. The gases in the interior of the bell were thus displaced by the incumbent column of water, and by moving the bell up and down repeatedly, a perfect equilibrium of temperature throughout the entire mass of water was quickly established. The temperature of the water was again carefully observed, and the difference between this and the previous observation determines the calorific power or potential energy, expressed as heat, of the substance consumed.

The value thus obtained is, however, obviously subject to the following corrections:—

1. The amount of heat absorbed by the calorimeter and apparatus employed, *to be added.*

2. The amount of heat carried away by the escaping gases, after issuing from the water, *to be added.*

3. The amount of heat due to the decomposition of the chlorate of potash employed, *to be deducted.*

4. The amount of heat equivalent to the work performed by the gases generated in overcoming the pressure of the atmosphere, *to be added.*

Although the errors due to these causes to some extent neutralize each other, there is still an outstanding balance of sufficient importance to require that the necessary corrections should be carefully attended to.

The amount of error from the first cause was once for all experimentally determined, and was added to the increase of temperature observed in each experiment.

The amount of heat carried away by the escaping gases after issuing from the water may be divided into two items, viz.:—

a. The amount of heat rendered latent by the water which is carried off by the gases in the form of vapor.

b. The amount of heat carried off by these gases by reason of their temperature being above that of the water from which they issue.

It was ascertained that a stream of dry air when passed through the water of the calorimeter, at about the same rate and for the same period of time as the gaseous products of combustion, depressed the temperature of the water by only 0°·02 C.

By placing a delicate thermometer in the escaping gases, and another in the water, no appreciable difference of temperature could be observed. Both these items may therefore be safely neglected.

The two remaining corrections can be best considered together, since a single careful determination eliminates both. When a combustible substance is burnt in gaseous oxygen, the conditions are essentially different from those which obtain when the same substance is consumed at the expense of the combined or solid oxygen of chlorate of potash. In the first case the products of combustion, when cooled to the temperature of the water in the calorimeter, occupy less space than the substances concerned in the combustion, and no part of the energy developed is therefore expended in external work, that is, in overcoming the pressure of the atmosphere. In the second case, both the combustible and the supporter of combustion are in the solid condition, whilst a considerable proportion of the products of combustion are gases. The generation of the latter cannot take place without the performance of external work, for every cubic inch produced must obviously, in overcoming atmospheric pressure, perform an amount of work equivalent, in round numbers, to the lifting of a weight of 15 lbs. to the height of one inch. In performing this work the gases are cooled, and consequently less heat is communicated to the water of the calorimeter. Nevertheless, the loss of heat due to this cause is but small. Under the actual conditions of the experiments detailed below, its amount would only have increased the temperature of the water in the calorimeter by 0°·07 C. Even this slight error is entirely eliminated by the final correction which we have now to consider.

It is well known that the decomposition of chlorate of potash into chlorid of potassium and free oxygen is attended with the evolution of heat. If a few grains of peroxyd of manganese, or better, of peroxyd of iron, be dropped into an ounce or two of fused chlorate of potash which is slowly disengaging oxygen, the evolution of gas immediately proceeds with great violence, and the mixture becomes visibly red hot, although the external application of heat be discontinued from the moment when the metallic peroxyd is added. The latter remains unaltered at the close of the operation. It is thus obvious that chlorate of potash, on being decomposed, furnishes considerably more heat than that which is necessary to gasify the oxygen which it evolves. It was therefore necessary to determine the amount of heat thus evolved by the quantity of chlorate of potash (9·75 grams) mixed with one gram of the substance burnt in each of the following determinations. This was effected by the use of two copper tubes, the one placed within the other. The interior tube was charged with a known weight of the same mixture of chlorate of potash and peroxyd of manganese as that used for the subsequent experiments, whilst the annular space between the two tubes was filled with a combustible mixture of chlorate and spermaceti, the calorific value of which had been previously ascertained. The latter mixture was ignited in the calorimeter as before, and the heat generated during its combustion effected the complete decomposition of the chlorate in the interior cylinder, as was proved by a subsequent examination of the liquid in the calorimeter, which contained no traces of undecomposed chlorate. The following are the results of five experiments thus made, expressed in units of heat, the unit being equal to 1 gram of water raised through 1° C. of temperature:—

	Units of Heat.
1st experiment, - - - - -	340
2nd " - - - - -	300
3rd " - - - - -	375
4th " - - - - -	438
5th " - - - - -	438
	5)1891
Mean, - - - -	378

This result was confirmed by the following experiments:—

1. Starch was burnt, firstly, in a current of oxygen gas, and secondly, by admixture with chlorate of potash and peroxyd of manganese.

Heat units furnished by one gram of starch burnt with 9·75 grams chlorate of potash, - - - - - - -	4290
Heat units furnished by the same weight of starch burnt in a stream of oxygen gas, - - - - - - - -	3964
Difference - - - - -	326

2d. Phenylic alcohol was burnt with chlorate of potash, and the result compared with the calorific value of this substance as determined by Favre and Silbermann.

Heat units furnished by one gram of phenylic alcohol burnt with 9·75 grams chlorate of potash, - - - - -	8183
Heat units furnished by one gram of phenylic alcohol when burnt with gaseous oxygen (Favre and Silbermann), - -	7842
Difference, - - - - - -	341

These three determinations of the heat evolved by the decomposition of 9·75 grams of chlorate of potash, furnishing the numbers 378, 326, and 341, agree as closely as could be expected, when it is considered that all experimental errors are necessarily thrown upon the calorific value of the chlorate of potash.

The mean of the above five experimental numbers was, in all cases, deducted from the actual values read off in the following determinations.

It was ascertained by numerous trials that all the chlorate of potash was decomposed in the deflagrations, and that but mere traces of carbonic oxyd were produced.

Joule's mechanical equivalent of heat was employed, viz., 1 kilogram of water raised 1° C.=423 meterkilograms.

The following results were obtained:

Actual energy developed by one gram of each substance when burnt in oxygen.

Name of substance dried at 100° C.	HEAT UNITS.					Meter-kilograms of force. (Mean.)
	1st Experiment.	2d Experiment.	3d Experiment.	4th Experiment.	Mean.	
Beef muscle purified by repeated washing with ether,	5174	5062	5195	5088	5103	2161
Purified albumen, ...	5009	4987	....	....	4998	2117
Beef fat, ...	9069	....	....	....	9069	3841
Hippuric acid, ...	5330	5437	....	....	5383	2280
Uric acid, ...	2645	2585	....	....	2615	1108
Urea,[11]	2121	2302	2207	2197	2206	934

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[11] The speaker showed the combustibility of urea by burning it upon asbestos in a jar of oxygen gas.

It is evident that the above determination of the actual energy developed by the combustion of muscle in oxygen represents more than the amount of actual energy produced by the oxydation of muscle within the body, because, when muscle burns in oxygen its carbon is converted into carbonic acid, and its hydrogen into water; the nitrogen being, to a great extent, evolved in the elementary state; whereas, when muscle is most completely consumed in the body, the products are carbonic acid, water and urea; the whole of the nitrogen passes out of the body as urea—a substance which still retains a considerable amount of potential energy. Dry muscle and pure albumen yield, under these circumstances, almost exactly one-third of their weight of urea, and this fact, together with the above determination of the actual energy developed on the combustion of urea, enables us to deduce with certainty the amount of actual energy developed by muscle and albumen respectively when consumed in the human body. It is as follows:—

Actual energy developed by one gram of each substance when consumed in the body.

Name of substance dried at 100° C.	Heat units. (Mean.)	Meterkilograms of force. (Mean.)
Beef muscle purified by ether,	4368	1848
Purified albumen, ...	4263	1803

We have thus ascertained the first of our three data, viz., the amount of force or actual energy generated by the oxydation of a given amount of muscle in the body; and we now proceed to ascertain the second, viz., the amount of mechanical force exerted by the muscles of the body during a given time. For this purpose we have only to avail ourselves of the details of Fick and Wislicenus's conclusive experiment already referred to, and which consisted in the ascent of the Faulhorn in Switzerland from the lake of Brienz. This mountain can be ascended by a very steep path from Iseltwald, which was of course favorable for the experiment, and there is a hotel on the summit which allowed the experimenters to pass the following night under tolerably normal circumstances. The following is their own description and estimate of the amount of work performed in the ascent.[12]

"Let us now inquire how much work was really done by our muscles. One item necessary for the reply is already at hand, viz., the height of the summit of the Faulhorn above the level of the lake of Brienz multiplied by the weight of the body; the former reckoned in meters, the latter in kilograms. The weight of the body with the equipments (hat, clothes, stick) amounted to 66 kilograms in Fick's case, and 76 in Wislicenus's. The height above the Faulhorn above the level of the lake of Brienz is, according to trigonometric measurements, exactly 1956 meters. Therefore Fick performed 129,096 and Wislicenus 148,656 meterkilograms of muscular work."

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[12] Phil. Mag., vol. xxxi, p. 496, 1866.

But in addition to this measurable external work there is another item of force "which can be expressed in units of work; and though its value cannot be quite accurately calculated, yet a tolerable approximation can be made. It consists of the force consumed in respiration and the heart's action. The work performed by the heart has been estimated, in a healthy full-grown man, at about 0·64 meterkilogram"[18] for each systole. During the ascent, Fick's pulse was about 120 per minute. That gives for the 5·5 hours of the ascent an amount of work which may be estimated at 25,344 meterkilograms, entirely employed in the maintenance of the circulation. No attempt has yet been made to estimate the labor of respiration. One of us has shown, however, in the second edition of his 'Medical Physics' (p. 206), that Donders's well-known investigations concerning the conditions of pressure in the cavity of the thorax give sufficient data for such an estimate. He has there shown that the amount of work performed in an inspiration of 600 cubic centims. may be rated at about 0·63 meterkilogram. Fick breathed during the ascent at an average rate of about 25 respirations per minute, which gives, according to this estimation, an amount of respiratory work for the whole ascent of 5197 meterkilograms. If we add this, and the number representing the work of the heart, to the external work performed by Fick, we obtain a total of 159,637 meterkilograms. If we suppose that Wislicenus's respiratory and circulatory work bore the same proportion to Fick's as his bodily weight did to Fick's, i. e., 7 : 6, we obtain for Wislicenus's amount of work, as far as it is possible to calculate it, a total of 184,287 meterkilograms.

"Besides these estimated (and certainly not over-estimated) items, there are several others which cannot be even approximately calculated, but the sum of which, if it could be obtained, would probably exceed even our present large total. We will try to give at least some sort of an account of them. It must first be remembered that in the steepest mountain path there are occasional level portions, or even descents. In traversing such places the muscles of the leg are exerted as they are in ascending, but the whole work performed is transformed back into heat. The same force-producing process, however, must be going on in the muscles as if work were being performed which did not undergo this transformation. In order to make this point yet clearer we may take into consideration that the whole work of the ascent, only existed temporarily as work. On the following day the result was reversed; our bodies approached the center of the earth by as much as they had receded from it the day before, and, in consequence, on the second day an amount of heat was liberated equal to the amount of work previously performed. The two parts of the action, which in this case were performed on two separate days, take place in walking on level ground in the space of a footstep.

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[18] 0·43 is here assigned as the work of the left, and 0·21 as that of the right ventricle.

"Let us observe, besides, that in an ascent it is not only those muscles of the leg specially devoted to climbing which are exerted, the arms, head, and trunk are continually in motion. For all these movements force-generating processes are necessary, the result of which cannot, however, figure in our total of work, but must appear entirely in the form of heat, since all the mechanical effects of these movements are immediately undone again. If we raise an arm, we immediately let it drop again, &c.

"There was besides a large portion of our muscular system employed during the ascent, which was performing no external work (not even temporary work, or mechanical effects immediately reversed), but which cannot be employed without the same force-generating processes which render external work possible. As long as we hold the body in an upright position, individual groups of muscles (as, for instance, the muscles of the back, neck, &c.) must be maintained in a state of continual tetanus in order to prevent the body from collapsing. We may conceive of a tetanized muscle as holding up a weight which would immediately fall if the supply of actual energy were to cease. It is active, but it performs no work, and therefore all the force produced is liberated in the form of heat."

Thus the total amount of measured and estimable work performed in 5·5 hours in the experiments before us was 159,637 meterkilograms for Fick, and 184,287 meterkilograms for Wislicenus. This is our second datum.

The third, viz., the amount of muscle oxydized in the body during the performance of this work has been carefully determined by the same experimenters, as well as the rate of muscle consumption before and after the ascent. For the details of these determinations the speaker referred his hearers to the Philosophical Magazine for 1866, vol. xxxi, p. 488; but the following is a condensed summary of the results:—

Ascent of the Faulhorn.

	Fick.	Wislicenus.
	Gram.	Gram.
Amount of nitrogen secreted in urine per hour before ascent,	·63	·61
Weight of dry muscle corresponding to nitrogen, ...	4·19	4·05
Amount of nitrogen secreted per hour during ascent,.........	·41	·39
Weight of dry muscle corresponding to nitrogen, ...	2·70	2·56
Amount of nitrogen secreted per hour during 6 hours after the ascent, ...	·40	·40
Weight of dry muscle corresponding to nitrogen, ...	2·63	2·63
Amount of nitrogen secreted per hour during the following night, ...	·45	51
Weight of dry muscle corresponding to nitrogen, ...	3·06	3·39
Total amount of nitrogen secreted during ascent, ...	3·31	3·13
Ditto during 6 hours after ascent, ...	2·43	2·42
	5·74	5·55
Weight of dry muscle corresponding to nitrogen secreted, During ascent, ...	20·98	20·89
During 6 hours after ascent,	16·19	16·11
	37·17	37·00

The results of these determinations add a new link to the chain of experimental evidence, that muscular exertion does not necessarily increase the excretion of nitrogen through the urine. From mid-day before the ascent (August 29th, 1865) to the following evening at seven o'clock (August 30th) both gentlemen abstained from all nitrogenous food. During these thirty-one hours they had nothing in the way of solid food except starch, fat, and sugar. The two former were taken in the form of cakes. Starch was made up with water into a thin paste, which was then made into small cakes and fried with plenty of fat. The sugar was taken dissolved in tea. In addition to this there was the sugar contained in the beer and wine, which were taken in quantities usual in mountain excursions. It was doubtless owing to this absence from food containing nitrogen that the amount of this element secreted through the urine, declined tolerably regularly from the 29th of August till the evening of the 30th. Even in the night of the 30th to the 31st, in spite of the plentiful meal of albuminous food on the evening of the 30th, the secretion of nitrogen was less than on the preceding night. The reason of this is probably to be sought for in the circumstance that during the period of abstinence, the secretion of nitrogen was carried on at the expense of tissues, and now these tissues required reparation.

It is perhaps scarcely worthy of record that during the ascent neither of the experimenters perspired perceptibly, since it has been proved by Ranke that no appreciable amount of nitrogen leaves the system in the matter of perspiration; and as Thiry has also shown that no nitrogen is got rid of by respiration, it follows that in addition to the nitrogen contained in the urine, the only other mode of exit for this element is through the fæces. Now the proportion secreted through the fæces has been estimated by Ranke at about one-twelfth of that in the urine; but inasmuch as all experiments on the subject tend to show that this alvine nitrogen is, as voided, a constituent of un-oxydized compounds, that is, of compounds that have not yielded up their force, it has no claim upon our attention.

There is still another circumstance which requires to be taken into consideration before we proceed to apply our three data to the solution of the problem before us. It is this:—Is it possible that at the termination of the ascent of the Faulhorn there might be a considerable quantity of the nitrogenous products of decomposition retained in the body? Considering the physiological effect of the retention of urea in the system, as exemplified whenever the secretion of urine is interrupted, it is difficult to imagine the possibility of any considerable quantity of urea being retained in the system of a healthy man. It is, however, otherwise with creatin, another of the products of the metamorphosis of tissue; for it has been repeatedly shown that a muscle which has been hard worked contains more creatin than one that has been at rest. Thus the quantity of creatin contained in the heart of an ox was found to be ·14 per cent (Gregory), and that in other ox-flesh only ·06 per cent (Staedeler). Now the muscles which extend the leg in walking, and which do the essential work in ascending, have been estimated by Weber to weigh in both legs 5·8 kilograms, and if we assume that before the ascent these muscles contained ·06 per cent of creatin, while after the ascent the percentage had increased to ·14 per cent, then the amount of creatin thus exceptionally retained would amount to 4·64 grams, which would be derived from 8·4 grams of muscle.

The speaker had been unable to determine the calorific effect of creatin, and consequently the actual energy developed by the transformation of muscle into creatin; for, although he was kindly furnished with an ample supply of this material by Dr. Dittmar, yet all attempts to burn it in the calorimeter were fruitless. Even when mixed in very small proportions with chlorate of potash and other combustibles of known value, the mixture invariably exploded violently on ignition. Although actual determination thus fails us, there can be no doubt that the transformation of muscle into creatin and other non-nitrogenous products must be attended by the liberation of far less actual energy than its transformation into urea, carbonic acid, and water. To be convinced of this, it is only necessary to compare (under equal nitrogen value) the formulæ of muscle, creatin, and urea, remembering at the same time that the nitrogen probably possesses no thermal value, and that each atom of oxygen destroys approximately the thermal effect of two atoms of hydrogen.

	Comparable formulæ.	Powerful or unburnt matter.
Muscle, - -	$Ꞓ_{24}H_{37}N_6\Theta_7$	$Ꞓ_{24}H_{23}$
Creatin, - -	$Ꞓ_8H_{18}N_6\Theta_4$	$Ꞓ_8H_{10}$
Urea, - -	$Ꞓ_3H_{12}N_6\Theta_3$	$Ꞓ_3H_6$

Thus it is evident that the amount of creatin exceptionally retained in the system could not greatly affect the result of the experiment as regards the possible amount of actual energy derivable from the metamorphosed tissues during the ascent; firstly, on account of the small quantity of creatin so retained, and, secondly, because creatin still contains about one-third of the potential energy of the muscle from which it is derived. But as this point cannot be experimentally demonstrated, the speaker followed the example of Fick and Wislicenus, and made a very liberal allowance on this score. He allowed, as they had done, that the whole of the nitrogen secreted during the six hours after the ascent was exceptionally retained in the system *as urea* during the ascent. This is equivalent to an admission that the muscles of the legs contained at the end of the ascent eleven times as much creatin as was present in them before the ascent. In the above tabular statement of results provision has been made for this allowance by adding together, on the one hand, the amounts of nitrogen secreted during the ascent and six hours after it, and, on the other, the weights of dry muscle corresponding to these two amounts of nitrogen.

Having thus far cleared the ground, let us now compare the amount of measured and calculated work performed by each of the experimenters during the ascent of the Faulhorn, with the actual energy capable of being developed by the maximum amount of muscle that could have been consumed in their bodies, this amount being represented by the total quantity of nitrogen excreted in each case during the ascent and for six hours afterwards.

	Fick.	Wislicenus.
	Grams.	Grams.
Weight of dry muscle consumed,...................	37·17	37·00
Actual energy capable of being produced by the consumption of 37·17 and 37·00 grams of dry muscle in the body,........................	Meterkilograms. 68,690	Meterkilograms. 68,376
Measured work performed in the ascent (external work),.....................................	129,096	. 148,656
Calculated circulatory and respiratory work performed during the ascent (internal work),.......	30,541	35,631
Total ascertainable work performed,...........	159,637	184,287

It is thus evident that the muscular power expended by these gentlemen in the ascent of the Faulhorn could not be exclusively derived from the oxydation, either of their muscles, or of other nitrogenous constituents of their bodies, since the maximum of power capable of being derived from this source even under very favorable assumptions is, in both cases, less than one-half of the work actually performed. But the deficiency becomes much greater if we take into consideration the fact, that the actual energy developed by oxydation or combustion cannot be wholly transformed into mechanical work. In the best constructed steam-engine, for instance, only one-tenth of the actual energy developed by the burning fuel can be obtained in the form of mechanical power; and in the case of man, Helmholtz estimates that not more than one-fifth of the actual energy developed in the body can be made to appear as external work. The experiments of Haidenhain, however, show that, under favorable circumstances, a muscle may be made to yield, in the shape of mechanical work, as much as one-half of the actual energy developed within it, the remainder taking the form of heat. Taking then this highest estimate of the proportion of mechanical work capable of being got out of actual energy, it becomes necessary to multiply by two the above numbers representing the ascertainable work performed, in order to express the actual energy involved in the production of that work. We then get the following comparison of the actual energy capable of being developed by the amount of muscle consumed, with the actual energy necessary for the performance of the work executed in the ascent of the Faulhorn.

	Fick.	Wislicenus.
	Meterkilograms.	Meterkilograms.
Actual energy capable of being produced by muscle metamorphosis, ...	68,690	68,376
Actual energy expended in work performed, ...	319,274	368,574

Thus, taking the average of the two experiments, it is evident that *scarcely one-fifth of the actual energy required for the work performed could be obtained from the amount of muscle consumed.*

Interpreted in the same way, previous experiments of a like kind prove the same thing, though not quite so conclusively. To illustrate this I will here give a summary of three sets of experiments: the first, made by Dr. E. Smith, upon prisoners engaged in treadmill labor; the second, by the Rev. Dr. Haughton, upon military prisoners engaged in shot drill; and the third, adduced by Playfair and made upon pedestrians, pile-drivers, men turning a winch, and other laborers.

Treadwheel experiments.—A treadwheel is a revolving drum with steps placed at distances of eight inches, and the prisoners are required to turn the wheel downwards by stepping upwards. Four prisoners, designated below as A, B, C, and D, were employed in these experiments, and each worked upon the wheel in alternate quarters of an hour, resting in a sitting posture during the intervening quarters. The period of actual daily labor was 3½ hours. The total ascent per hour 2160 feet, or per day 1·432 mile. The following are the results:—

Treadwheel work.—(E. Smith.)

	Weight in kilograms.	Ascent in meters.	Days occupied in ascent.	External work performed in meterkilograms.	Total nitrogen evolved.	Weight of dry muscle corresponding to nitrogen.
					Grams.	Grams.
A	47·6	23,045	10	1,096,942	171·3	1101·2
B	49	23,045	10	1,129,205	174·5	1121·7
C	55	20,741	9	1,140,755	168·0	1080·1
D	56	20,741	9	1,161,496	159·3	1024·3

In these experiments the measured work was performed in the short space of 3½ hours, while the nitrogen estimated was that voided in the shape of urea in 24 hours. It will, therefore, be necessary to add to the measured work that calculated for respiration and circulation for the whole period of 24 hours. This amount of internal work was computed, from the estimates of Helmholtz and Fick, to be as follows:—

Internal work.—(Helmholtz and Fick.)

	Work performed.	Actual energy required.
	Meterkilograms.	Meterkilograms.
Circulation of the blood during 24 hours, at 75 pulsations per minute,	69,120[14]	138,240
Respiration for 24 hours, at 12 respirations per minute,	10,886	21,772
Statical activity of muscles,	not determined.	not determined,
Peristaltic motion,	" "	" "
	80,006	160,012

Taking this estimate for internal work, the average results of the treadwheel experiments may be thus expressed:—

Treadwheel work.

Average external work per man per day,		119,605 mks.
Average nitrogen evolved per man per day,		17·7 grams.
Weight of dry muscle corresponding to average nitrogen evolved per day,		114 "
Actual energy producible by the consumption of 114 grams of dry muscle in the body,		210,672 mks.
Average actual energy developed in the body of each man, viz.—		
External work,	119,605×2=239,210 mks.	
Circulation,	69,120×2=138,240 "	
Respiration,	10,886×2= 21,772 "	
		399,222 "

In these experiments the conditions were obviously very unfavorable for the comparison of the amount of actual energy producible from muscle metamorphosis, with the quantity of actual energy expended in the performance of estimable work;

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[14] Since making use of this number, I find that Donders estimates the work of the heart alone, for 24 hours, at 86,000 meterkilograms, a figure which is higher than that above for the combined work of circulation and respiration.

since, during that portion of the twenty-four hours not occupied in the actual experiment, a large amount of unestimable internal work, such as the statical activity of the muscles, peristaltic motion, &c., was being performed. Nevertheless, these experiments show that the average actual energy developed in producing work in the body of each man was nearly twice as great as that which could possibly be produced by the whole of the nitrogenous matter oxydized in the body during 24 hours. It must also be remarked that the prisoners were fed upon a nitrogenous diet containing six ounces of cooked meat, without bone; a diet which, as is well known, would favor the production of urea.

Shot-drill experiments.—The men employed for these experiments were fed exclusively upon vegetable diet, and they consequently secreted a considerably smaller amount of nitrogen than the flesh-eaters engaged in the treadwheel work. The other conditions were, however, equally unfavorable for showing the excess of work performed, over the amount derivable from muscle metamorphosis.

In shot-drill, each man lifts a 32 lb. shot from a tressel to his breast, a height of 3 feet; he then carries it a distance of 9 feet, and lays it down on a similar support, returning unloaded. Six of these double journeys occupy one minute. The men were daily engaged with—shot-drill 3 hours, ordinary drill 1¼ hours, oakum picking 3½ hours.

The total average daily external work was estimated by Haughton at 96,316 meterkilograms per man.

The following is a condensed summary of the results of these experiments:—

Military vegetarian prisoners at shot-drill.—(Haughton.)

Average external work per man per day,		96,316 mks.
Average nitrogen evolved per man per day,		12·1 grams.
Weight of dry muscle corresponding to average nitrogen evolved per day,		77·9 "
Actual energy producible by the consumption of 77·9 grams of dry muscle in the body,		143,950 mks.
Average actual energy developed daily in the body of each man, viz., External work, 96,316×2=	192,632 mks.	
Internal work,	160,012 "	
		352,644 mks.

Owing chiefly to the vegetable diet of these prisoners, the result is more conclusive than that obtained upon the treadwheel, the amount of work actually performed being considerably more than twice as great as that which could possibly be obtained through the muscle metamorphosis occurring in the bodies of the prisoners.

Playfair's determinations.—In these determinations the number 109,496 meterkilograms was obtained as the average amount of daily work performed by pedestrians, pile-drivers, porters, paviors, &c.; but, as the amount of muscle consumption is calculated from the nitrogen taken in the food, the conditions are as unfavorable as possible with regard to the point the speaker was seeking to establish; for it is here assumed, not only that all the nitrogen taken in the food enters the blood, but also that it·is converted into muscle, and is afterwards oxydized to carbonic acid, water, and urea. The following are the results expressed as in the previous cases:—

Hard-worked laborer.—(Playfair.)

	Work performed.	Actual energy required.
Daily labor (external work), - - -	109,496 mks.	218,992 mks.
Internal work, - - - - - -	80,006 "	160,012 "
	189,502 mks.	379,004 mks.
Actual energy capable of being produced from 5·5 oz. (155·92 grams) of flesh-formers contained in the daily food of the laborer, -	....	288,140 mks.

Thus, even under the extremely unfavorable conditions of these determinations, the actual work performed exceeded that which could possibly be produced through the oxydation of the nitrogenous constituents of the daily food by more than 30 per cent.

We have seen, therefore, in the above four sets of experiments, interpreted by the data afforded by the combustion of muscle and urea in oxygen, that the transformation of tissue alone cannot account for more than a small fraction of the muscular power developed by animals; in fact, this transformation goes on at a rate almost entirely independent of the amount of muscular power developed. If the mechanical work of an animal be doubled or trebled there is no corresponding increase of nitrogen in the secretions; whilst it was proved on the other hand by Lawes and Gilbert, as early as the year 1854, that animals, under the same conditions as regarded exercise, had the amount of nitrogen in their secretions increased twofold by merely doubling the amount of nitrogen in their food. Whence then comes the muscular power of animals? What are the substances which, by their oxydation in the body, furnish the actual energy, whereof a part is converted into muscular work? In the light of the experimental results detailed above, can it be doubted that a large proportion of the muscular power developed in the bodies of animals has its origin in the oxydation of non-nitrogenous substances? For while the secretion of nitrogen remains nearly stationary under widely different degrees of muscular exertion, the production of carbonic acid increases most markedly with every augmentation of muscular work, as is shown by the following tabulated results of E. Smith's highly important experiments regarding the amount of carbonic acid evolved from his own lungs under different circumstances.[15]

Excretion of carbonic acid during rest and muscular exertion:—

	Carbonic acid per hour.
During sleep,	19·0 grams.
Lying down and sleep approaching,	23·0 "
In a sitting posture,	29·0 "
Walking at rate of 2 miles per hour,	70·5 "
" " 3 " "	100·6 "
On the treadwheel, ascending at the rate of 28·65 feet per minute,	189·6 "

It has been already stated as a proposition upon which all are agreed, that food, and food alone, is the ultimate source from which muscular power is derived; but the above determinations and considerations, the speaker believed, prove conclusively, firstly, that the non-nitrogenous constituents of the food, such as starch, fat, &c., are the chief sources of the actual energy, which becomes partially transformed into muscular work; and secondly, that the food does not require to become organized tissue before its metamorphosis can be rendered available for muscular power; its digestion and assimilation into the circulating fluid—the blood—being all that is necessary for this purpose. It is, however, by no means the non-nitrogenous portions of food alone that are capable of being so employed, the nitrogenous also, inasmuch as they are combustible, and consequently capable of furnishing actual energy, might be expected to be available for the same purpose, and such an expectation is confirmed by the experiments of Savory upon rats,[16] in which it is proved that these animals can live for weeks in good health upon food consisting almost exclusively of muscular fibre. Even supposing these rats to have performed no external work, nearly the whole of their internal muscular work must have had its source in the actual energy developed by the oxydation of their strictly nitrogenous food.

It can scarcely be doubted, however, that the chief use of the nitrogenous constituents of food is for the renewal of muscular tissue; the latter, like every other part of the body, requiring a continuous change of substance, while the chief function of the non-nitrogenous is to furnish by their oxydation the actual energy which is in part transmuted into muscular force.

The combustible food and oxygen coëxist in the blood which courses through the muscle, but when the muscle is at rest there is no chemical action between them. A command is sent from the brain to the muscle, the nervous agent determines oxydation. The potential energy becomes active energy, one portion assuming the form of motion, another appearing as heat. *Here is the*

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[15] Phil. Trans. for 1859, p. 709.

[16] The Lancet, 1863, pages 381 and 412.

source of animal heat, here the origin of muscular power! Like the piston and cylinder of a steam-engine, the muscle itself is only a machine for the transformation of heat into motion; both are subject to wear and tear and require renewal, but neither contributes in any important degree by its own oxydation to the actual production of the mechanical power which it exerts.

From this point of view it is interesting to examine the various articles of food in common use, as to their capabilities for the production of muscular power. The speaker had therefore made careful estimations of the calorific value of different materials used as food, by the same apparatus and in the same manner as described above for the determination of the actual energy in muscle, urea, uric acid, and hippuric acid.

The results are embodied in the following series of tables, but it must be borne in mind that it is only on the condition that the food is digested and passes into the blood, that the results given in these tables are realized. If, for instance, sawdust or paraffin oil had been experimented upon, numbers would have been obtained for these substances, the one about equal to that assigned to starch, and the other surpassing that of any article in the table; but these numbers would obviously have been utterly fallacious, inasmuch as neither sawdust nor paraffin oil is, to any appreciable extent, digested in the alimentary canal. While the force-values experimentally obtained for the different articles in these tables must therefore be understood as the maxima assignable to the substances to which they belong, yet it must not be forgotten that a large majority of these substances appear to be completely digestible under normal circumstances.

Actual energy developed by one gram of various articles of food when burnt in oxygen.

Name of food.	Heat units.		Meterkilograms of force.		Per cent of water.
	Dry.	Natural condition.	Dry.	Natural condition.	
Cheese (Cheshire),	6114	4647	2589	1969	24·0
Potatoes,	3752	1013	1589	429	73·0
Apples,	3669	600	1554	280	82·0
Oatmeal,	....	4004	....	1696	...
Flour,	....	3941	....	1669	...
Pea-meal,	....	3936	....	1667	...
Ground rice,	....	3813	....	1615	...
Arrowroot,	....	3912	....	1657	...
Bread crumb,	3984	2231	1687	945	44·0
" crust,	....	4459	....	1888	...
Beef (lean),	5313	1567	2250	664	70·5
Veal "	4514	1314	1912	556	70·9
Ham "	4343	1980	1839	839	54·4
Mackerel,	6064	1789	2568	758	70·5
Whiting,	4520	904	1914	383	80·0
White of egg,	4896	671	2074	284	86·3
Hard-boiled egg,	6321	2383	2677	1009	62·3

Actual energy developed by one gram of various articles of food—continued.

Name of food.	Heat units.		Meterkilograms of force.		Per cent of water.
	Dry.	Natural condition.	Dry.	Natural condition.	
Yolk of egg,	6460	3423	2737	1449	47·0
Gelatin,	4520	....	1914	....	...
Milk,	5093	662	2157	280	87·0
Carrots,	3767	527	1595	223	86·0
Cabbage,	3776	434	1599	184	88·5
Cocoa nibs,	....	6873	....	2911	...
Beef fat,	9069	....	3841	....	...
Butter,	....	7264	....	3077	...
Cod-liver oil,	....	9107	....	3857	...
Lump sugar,	....	3348	....	1418	...
Commercial grape sugar,	....	3277	....	1388	...
Bass's ale (alcohol reckoned),	3776	775	1599	328	88·4
Guinness's stout,	6348	1076	2688	445	88·4

Actual energy developed by one gram of various articles of food when oxydized in the body.

Name of food.	Meterkilograms of force.		Name of food.	Meterkilograms of force.	
	Dry.	Natural condition.		Dry.	Natural condition.
Cheshire cheese,	2429	1846	Hard-boiled egg,	2562	966
Potatoes,	1563	422	Yolk of egg,	2641	1400
Apples,	1516	273	Gelatin,	1550	....
Oatmeal,	....	1665	Milk,	2046	266
Flour,	....	1627	Carrots,	1574	220
Pea-meal,	....	1598	Cabbage,	1543	178
Ground rice,	....	1591	Cocoa nibs,	....	2902
Arrowroot,	....	1657	Butter,	....	3077
Bread crumb,	1625	910	Beef fat,	3841	....
Lean of beef,	2047	604	Cod-liver oil,	3857	....
" veal,	1704	496	Lump sugar,	....	1418
" ham, boiled,	1559	711	Commercial grape sugar,	....	1388
Mackerel,	2315	683	Bass's ale, bottled,	1559	328
Whiting,	1675	335	Guinness's stout,	2688	455
White of egg,	1781	244			

Weight and cost of various articles of food required to be oxydized in the body in order to raise 140 *lbs. to the height of* 10,000 *feet.*

External work = ⅕th actual energy.

Name of food.	Weight in lbs. required.	Price per lb.		Cost.	
		s.	*d.*	*s.*	*d.*
Cheshire cheese,	1·156	0	10	0	11½
Potatoes,	5·068	0	1	0	5¼
Apples,	7·815	0	1½	0	11¾
Oatmeal,	1·281	0	2¾	0	3½
Flour,	1·311	0	2¾	0	3¾
Pea-meal,	1·335	0	3¼	0	4½
Ground rice,	1·341	0	4	0	5½
Arrowroot,	1·287	1	0	1	3½
Bread,	2·345	0	2	0	4¾
Lean beef,	3·532	1	0	3	6½
" veal,	4·300	1	0	4	3½

Weight and cost of various articles of food—continued.

Name of food.	Weight in lbs. required.	Price per lb.		Cost.	
		s.	*d.*	*s.*	*d.*
Lean ham, boiled,	3·001	1	6	4	6
Mackerel,	3·124	0	8	2	1
Whiting,	6·369	1	4	9	4
White of egg,	8·745	0	6	4	4½
Hard-boiled egg,	2·209	0	6½	1	2½
Isinglass,	1·377	16	0	22	0½
Milk,	8·021	5*d.* per quart.		1	3½
Carrots,	9·685	0	1½	1	2½
Cabbage,	12·020	0	1	1	0¼
Cocoa-nibs,	0·735	1	6	1	1¼
Butter,	0·693	1	6	1	0½
Beef fat,	0·555	0	10	0	5½
Cod-liver oil,	0·553	3	6	1	11¼
Lump sugar,	1·505	0	6	1	3
Commercial grape sugar,	1·537	0	3½	0	5½
Bass's pale ale (bottled),	9 bottles.	0	10	7	6
Guinness's stout,	6¾ "	0	10	5	7½

Weight of various articles of food required to sustain respiration and circulation in the body of an average man during 24 hours.

Name of food.	Weight in oz.	Name of food.	Weight in oz.
Cheshire cheese,	3·0	Whiting,	16·8
Potatoes,	13·4	White of egg,	28·1
Apples,	20·7	Hard-boiled egg,	5·8
Oatmeal,	3·4	Gelatine,	3·6
Flour,	3·5	Milk,	21·2
Pea-meal,	3·5	Carrots,	25·6
Ground rice,	3·6	Cabbage,	31·8
Arrowroot,	3·4	Cocoa-nibs,	1·9
Bread,	6·4	Butter,	1·8
Lean beef,	9·3	Cod-liver oil,	1·5
" veal,	11·4	Lump sugar,	3·9
" ham, boiled,	7·9	Commercial grape sugar,	4·0
Mackerel,	8·3		

These results are in many instances fully borne out by experience. The food of the agricultural laborers in Lancashire contains a large proportion of fat. Besides the very fat bacon which constitutes their animal food proper, they consume large quantities of so-called apple dumplings, the chief portion of which consists of paste in which dripping and suet are large ingredients, in fact these dumplings frequently contain no fruit at all. Egg and bacon pies and potato pies are also very common *pièces de résistance* during harvest-time, and whenever very hard work is required from the men. The speaker well remembers being profoundly impressed with the dinners of the navigators employed in the construction of the Lancaster and Preston Railway; they consisted of thick slices of bread surmounted with massive blocks of bacon, in which mere streaks of lean were visible. Dr. Piccard states that the Chamois hunters of Western Switzerland are accustomed, when starting on long and fatiguing expeditions, to take with them, as provisions, nothing but bacon-fat and sugar, because, as they say, these substances are more nourishing than meat. They doubtless find that in fat and sugar they can most conveniently carry with them a store of force-producing matter. The above tables affirm the same thing. They show that ·55 lb. of fat will perform the work of 1·15 lb. cheese, 5 lbs. potatoes, 1·3 lb. of flour or pea-meal or of 3½ lbs. of lean beef. Donders, in his admirable pamphlet 'On the Constituents of Food and their Relation to Muscular Work and Animal Heat,' mentions the observations of Dr. M. C. Verloren on the food of insects. The latter remarks, "Many insects use during a period in which very little muscular work is performed food containing chiefly albuminous matter; on the contrary, at a time when the muscular work is very considerable, they live exclusively, or almost exclusively, on food free from nitrogen." He also mentions bees and butterflies as instances of insects performing enormous muscular work, and subsisting upon a diet containing but the merest traces of nitrogen.

We thus arrive at the following conclusions:—

1. The muscle is a machine for the conversion of potential energy into mechanical force.

2. The mechanical force of the muscles is derived chiefly, if not entirely, from the oxydation of matters contained in the blood, and not from the oxydation of the muscles themselves.

3. In man the chief materials used for the production of muscular power are non-nitrogenous; but nitrogenous matters can also be employed for the same purpose, and hence the greatly increased evolution of nitrogen under the influence of a flesh diet, even with no greater muscular exertion.

4. Like every other part of the body, the muscles are constantly being renewed; but this renewal is not perceptibly more rapid during great muscular activity than during comparative quiescence.

5. After the supply of sufficient albuminized matters in the food of man to provide for the necessary renewal of the tissues, the best materials for the production, both of internal and external work, are non-nitrogenous matters, such as oil, fat, sugar, starch, gum, &c.

6. The non-nitrogenous matters of food, which find their way into the blood, yield up all their potential energy as actual energy; the nitrogenous matters, on the other hand, leave the body with a portion (one-seventh) of their potential energy unexpended.

7. The transformation of potential energy into muscular power is necessarily accompanied by the production of heat within the body, even when the muscular power is exerted externally. This is doubtless the chief and, probably, the only source of animal heat.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *Apparatus for the direct determination of the velocity of sound in atmospheric air;* by Dr. E. C. O. Neumann.—This ingenious little apparatus consists of a box of wood (82 c.m. long, 66 c.m. wide, 7 c.m. high) divided by vertical partitions in such a manner as to form two canals, running from one corner, F, of the box, the first only to the middle, the other by a winding course to near the same point so as to be about six meters longer. A little gun is placed on top of the box; when fired the sound is conveyed through a square tube to the corner F, where it divides; one part goes directly to the middle, A, of the front side of the box, and pushing a short wire, *f*, fastened upon a thin caoutchouc membrane by means of a small piece of wood; the other part of the sound traverses the six meters of winding tube, and finally strikes the membrane, closing an aperture, B (near A)—the latter membrane likewise being provided with a blackened wire, *g* (1 mm. diam.). In front of these wires a disk, covered with white paper, can be made to rotate around a horizontal axis.

When this disk is at rest the report passing in at F will soon push the wire *f* against the paper, leaving a black spot—and when the six meters have been traversed, the other part of the sound wave will push the wire *g* against the disk, in the same distance of 15 c.m. from the axis of rotation. But when the disk made one turn per second, these two marks on the circumference were 16⅓ mm. farther apart than when the disk was at rest. Then we evidently have the velocity, *v*, of sound by the proportion

$$v : 2\pi 15\text{c.m.} :: 6\text{m.} : 16\tfrac{1}{3}\text{mm.},$$

or $v = 346^{\text{m}}{\cdot}2$. The temperature was 22° C.; a good approximation for a first trial.—*Pogg. Ann.*, 1866, cxxiii, 307–311. G. H.

2. *Interference apparatus for sound-waves;* G. Quincke.—This apparatus is based upon Herschel's idea of applying branching tubes, and admits of subjective and objective experimentation; it also may be applied to the study of secondary tones (ober-tone), etc., like Helmholtz's resonators. The simplest kind consists of two bent glass tubes DCBEF and GHILM provided with a branch BA and IK. At A is a short rubber tube, which is put into one ear while the other is well closed; at K is another rubber tube leading to a source of sound, viz., the vibrating branch of a tuning fork or its middle rod, or into the box of a monochord, etc. D and G are connected by a short rubber tube; between F and M a longer one is inserted, so as to make ILEB an uneven number of half wave-lengths longer than IHCB. If a tuning fork is used, and the length of MF properly adjusted (the interference-tube tuned), then only the octave is heard, the prime being destroyed by interference. If the rubber tube FM or DG is closed by pressure with the finger, the fundamental tone of the tuning fork is heard again.

C D G H
A B I K
E F M L

For objective representation A is connected with a glass bell closed by a fine membrane, the bell and membrane tuned in unison with the tone experimented with; the sand on the membrane will not be moved when the tone-wave passes through both branches, but it will move as soon as one of the branches is closed.

The same apparatus may also be connected with one of Kundt's tubes described at p. 258 of this volume; and it has finally the great advantage that it can be very easily made by almost anyone.—*Pogg. Ann.*, 1866, cxxviii, 177–192. G. H.

3. *A new apparatus for the demonstration of the laws of falling bodies.* ("Fall-machine;" might it not be rendered "fall-apparatus?")—F. Lippich of Gratz, Austria, has constructed an elegant, simple, and compendious little apparatus for the above purpose, admitting of a much higher degree of accuracy than either Atwood's or Morin's apparatus, and at the same time furnishing us with a drawing representing the laws.

It consists essentially of four parts. A vertical *support* (about 20 inches high) which can be attached to a table; by means of a fine string a *fall frame* (about 15 inches high) is suspended to the same; an elastic vertical *spring*, attached to the support by passing through its position of equilibrium, opens the very ingenious *clamp* holding the string, so that the frame commences its descent at the very moment the spring passes the vertical position. The light frame is covered with well-stretched sooted paper; the vertical spring oscillating parallel to this paper carries a fine point, which marks a wave-line on the descending paper. The oscillations of the spring giving equal times, the intersections of the above curve with the vertical will, and do very accurately, give the spaces described in the times 1, 2, 3, etc. Drawing tangents at these points of intersection the velocities are determined. Measuring the exact length of the fall during six oscillations gave $296{\cdot}24^{mm}$, $296{\cdot}05^{mm}$, $296{\cdot}26^{mm}$, in three experiments. The spring vibrating four times for about fourteen seconds in front of a uniformly moving paper strip, gave a mean of 24·414 vibrations per second. These values give directly $g=9{\cdot}80935$, 9·81007, and 9·80310, or the mean $9{\cdot}80751^{m}$, while the value calculated from $g=9{\cdot}80557\ (1-0{\cdot}002588\cos 2\varphi)$ for Prague, where the experiments were made, and $\varphi=50^{\circ}\ 5'\ 19''{\cdot}22$, give $g=9{\cdot}81005^{m}$.

Lippich has also experimented upon the influence of the resistance of the air by attaching horizontal card-boards of various sizes to the fall-frame; and finally refers to an apparatus of Laborde based upon the same idea, but less perfect in its realization.—*Sitzungsberichte*, Wien, 1865, II Abth., Bd. lii, p. 549–562; *L'Institut*, 1866, p. 199. G. H.

1. *Astro-photometer and results obtained;* by Dr. J. C. F. Zöllner.—The astro-photometer is described in Zöllner's "Grundzüge einer allgemeinen Photometrie des Himmels, Berlin, 1861." It is polarizing, making use of the law of the square of the cosine as the intensity transmitted through a Nicol. The light of a kerosene-flame, F, passes through a round opening, *a*, to a biconcave lens, *b*, thence to a Nicol *c*, quartz-plate, *f*, Nicol's *d* and *e*, through a double convex lens, *g*, to a plane glass plate inclined 45° toward these rays and the axis of the telescope, *Ok*, reflecting the image of *a* to *i* near that of the star seen in the telescope at *k*. The rotation of Nicol *c* is read off on the graduated circle *m*,

that of *d* on *l*; by rotation of *c* the *color*, by turning of *d* the intensity of the flame is made equal to that of the star. This photometer can be attached to most telescopes.

In his "Photometrische Untersuchungen mit besonderer Rücksicht auf die physische Beschaffenheit der Himmelskörper, Leipzig, 1865," Zöllner has given the *results* of his observations together with their bearing upon the theory of Kant-Laplace. The following intensities were obtained by comparing the sun or planets separately with α Aurigæ; he found

Sun : Capella : : 55,760,000,000 : 1

with a probable error of about 5 per cent; and hence for the intensity at the mean opposition,

		Prob. error.
Sun =	6,994,000,000 times Mars,	5·8 p. c.
Sun =	5,472,000,000 " Jupiter,	5·7 "
Sun =	130,980,000,000 " Saturn (without the ring)	5·0 "
Sun =	8,486,000,000,000 times Uranus,	6 0 "
Sun =	79,620,000,000,000 " Neptune,	5·5 "
Sun =	619,600 " Full Moon,	2·7 "

and by comparing surfaces, Sun = 618,000 times Full Moon, 1·6 p. c.

From the above it follows, that our sun at a distance of 3·72 years-way of light would appear like Capella with a parallax of 0·874 seconds. Peters has actually found 0″·046. If light suffers no absorption in the celestial spaces, Capella accordingly must send out much more light than our sun; and α Centauri seems to be equal to our sun.

The reflecting power or albedo Zöllner found as follows:

		Prob. error.			Prob. error.
Moon,	0·1736	±0·0035	Saturn,	0·4981	±0·0249
Mars,	0·2672	±0·0155	Uranus,	0·6400	±0·0544
Jupiter,	0·6238	±0·0355	Neptune,	0·4648	±0·0372

For the sake of comparison we add his determination of the albedo of terrestrial substances: (*a.*) diffuse reflected light—snow just fallen 0·783, white paper 0·700, white sandstone 0·237, clay-marl 0·156, quartz-porphyry 0·108, moist soil 0·079, dark gray syenite 0·078. (*b.*) regular reflection—mercury 0·648, speculum metal 0·535, glass 0·040, obsidian 0·032, water 0·021.

In regard to the intensity of lunar light in the different phases we must refer for the theoretical investigation to Zöllner's paper in Pogg. Ann., 1866, vol. cxxviii, pp. 46–61; and for the results to pp. 260–262.

In his interesting theoretical views Zöllner distinguishes five periods in the history of any star-sun; viz., 1st, *glowing-gaseous* condition; 2d, *glowing-liquid*; 3d, *slag-period*, gradual development of a cool non-luminous surface; 4th, *eruption-period*, bursting of the cool and dark shell, and 5th, *period of complete refrigeration.* These periods he finds again in the cosmical history of the earth, and in the present aspect of the starry heavens. As representatives of the first period he considers the planetary nebulæ; the other nebulæ stand between the first and second period, which latter is represented by the invariable stars; our *sun* is in the third period: to the fourth belong the new stars; and the fifth period

sel's dark stars. For a full exposition of these hy-

to the above-mentioned works of Zöllner. G. H.

II. MINERALOGY AND GEOLOGY.

1. *Note on the possible identity of Turnerite with Monazite ;* by J. D. Dana.—The crystals of the rare mineral Turnerite have been measured by Levy (who first described the species), Marignac, Phillips, Descloizeaux, and vom Rath. The latest investigations, by Dr. G. vom Rath, are published in Poggendorff's Annalen, vol. cxix, p. 247, and are accompanied with two new figures; his crystals were from a new locality in the Tavetsch valley, at Santa Brigritta near Ruäras—the specimens before known having come from Mt. Sorel in Dauphiny. The crystals are somewhat tabular, with (1) a zone parallel to the orthodiagonal of the three planes, in order, x, c, u, and a fourth a but only as a result of cleavage, on the edge $x : u'$; and (2) a transverse zone, directly across c, containing, either side of c, the planes n, v, e, o, b, the last the face $i\text{-}i$ (($\infty P \infty$) of Naumann) parallel to the clinodiagonal section; also (3) some other planes. Vom Rath makes $c = O$(oP of Naumann); a (cleavage face) $= i\text{-}i$ ($\infty P \infty$); $u = -1\text{-}i$ ($-P \infty$); $x = 1\text{-}i$ ($+P \infty$); n, v, e, o, the clinodomes $\frac{1}{8}\text{-}\grave{\imath}$, $\frac{1}{2}\text{-}\grave{\imath}$, $1\text{-}\grave{\imath}$, $2\text{-}\grave{\imath}$.

The following are a few of the angles given:

		v. Rath.	Descl.	Marignac.
$a : x$	=	130° 3′	....	....
$a : u$	=	142° 15′	....	....
$c : u$	=	140° 27′	140° 40′	....
$c : x$	=	127° 15′	126° 31′	126° 31′
$c : e$	=	136° 55′	136° 48′	136° 43′

In form and habit the crystals are much like those of monazite, and there is also a close approximation to that species in angle. By changing the position of the crystal so that $a = O$ and $c = i\text{-}i$, we have that usually given to the crystals of monazite. The *cleavage face* corresponds to O of monazite (see my Mineralogy, p. 402); $c = i\text{-}i$, $u = 1\text{-}i$, $x = -1\text{-}i$; n, v, e, o, are vertical prisms; and $e = I$, or the fundamental prism. The angles in monazite corresponding to the above are as follows:

$O : -1\text{-}i = a : x =$ 136° 6′
$O : 1\text{-}i = a : u =$ 143° 6′
$ii : 1\text{-}i = c : u =$ 140° 40′
$ii : -1\text{-}i = c : x =$ 126° 8′ (127° 0′, Descl.)
$u : I = c : e =$ 136° 40′ (136° 30′, Descl.)

The angles cited are sufficient to determine all the dimensions of the crystals; and the approximations in angle and cleavage leave little doubt of at least the near identity in crystallization of turnerite and monazite. The absence of a plane corresponding to the cleavage direction is another mark of resemblance. Moreover, in hardness they are the same; in color very similar. Yet the actual identity of the species cannot be considered as settled without new crystallographic comparisons, or a chemical examination of turnerite. The trials by Mr. Children were too imperfect to be decisive against it; while they show that turnerite is not a titanate or silicate.

2. *Grahamite.*—Professor Henry Wurtz has proposed the name *Grahamite* (Report upon a Mineral Formation in West Virginia: New York,

1865) for the pitch-black Albertite-like mineral of Virginia described by J. P. Lesley (see this Jour., xxxvii, 149). Mr. Lesley took the ground that it was not true coal, and compared it to Albertite. We gather from Mr. Wurtz's Report the following facts. The vein occurs in Ritchie Co., in Carboniferous rocks, and occupies a *shrinkage* fissure. It is about 4½ feet wide; 2 inches outside are granular; the next 15 or 16 inches columnar and very lustrous; the middle, averaging 18 inches, though varying much, less columnar and less lustrous, and more resinous in fracture. He concludes that the fissure was filled by the exudation of the resinoid substance while it was in a pasty condition. G.=1·145. An analysis by Dr. J. Maier afforded C 76·45, H 7·82, O (with traces of N) 13·46, ashes 2·26=100. No action with cold or melted caustic potash, or boiling nitric or muriatic acid, or aqua regia; a brown solution with sulphuric acid. Wholly insoluble in alcohol; partly soluble in naphtha, benzole and ether; almost wholly so and readily in chloroform and sulphuret of carbon; mostly so, but slowly, in oil of turpentine. At 400° F. it begins to decrepitate, smoke and soften, and gives off water; above this, empyreumatic vapors and a pasty fusion internally, the exterior decomposing; and when drawn apart in this state it forms long delicate threads. Mr. Wurtz found that, under the same circumstances, *Albertite* might be drawn into threads.

The Report closes with suggestions as to methods of utilizing grahamite in the manufacture of illuminating oils and gas, a cement for sealing bottles (for which its indifference to acids and alkalies especially adapts it), translucent varnishes, lubricating compositions, etc.

3. *On the discovery of Corundum at the Emery mine, Chester, Mass.;* by Dr. C. T. Jackson. (From a letter to one of the Editors.)—At the middle of July last I found a perfect crystal of blue corundum or sapphire at the Chester mine. This crystal is among the specimens which I have arranged for the Emery company to send to the Paris Exposition. It is surrounded by magnesian carbonate of lime or crystallized dolomite. The form of the crystal is that of the double pyramid with six planes—like that figured by Dufrénoy on page 49, fig. 303; and it is three-tenths of an inch long.

4. *Note concerning the minerals of the Emery mine of Chester, Mass.;* by Prof. C. U. Shepard. (Communicated for this Journal.)—There are obviously two chloritic species in addition to the chloritoid found at Chester. The chloritoid analyzed by Dr. Jackson, of which an account was published in the last number of this Journal, is not the mineral[1] designated as such by me as occurring in the emery vein of the north mountain,[2] but the softer clinochlore. I at first supposed it to be corundophilite. The analysis refers to the same mineral as analyzed by Prof. J. L. Smith and quoted in the same number of the Journal. Both are analyses of a mineral whose crystallization, hardness and gravity, leave but little room to doubt that it is clinochlore; but the substance called the "fringe-rock," attached to both sides of a feldspathic vein of which I had said it was difficult to say "whether it is chlorite or corundophilite," cannot mineralogically belong to biotite, as Dr. Smith thinks, inasmuch

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[1] That mineral is close to masonite or ottrelite.

[2] See p. 10 of my Report on the Emery mine of Chester. London, 1865.

as its hardness is only 1·5, whereas if biotite it should be from 2·5 to 3·0. Besides it is flexible and wholly inelastic. Nor can these differences arise from decomposition, for the mineral is perfectly fresh, shining and translucent. Its specific gravity is 2·76. Before the blowpipe it hardens, and then melts with the greatest difficulty upon thin edges into a pale bottle-green glass. It appears to me, therefore, to approach very closely to the Buncombe mineral found with sapphire, and called by me, corundophilite.[3]

Rutile.—This is rather frequent, in bright red, slender and much striated prisms, closely associated with diaspore and clinochlore, sometimes in reticulated aggregations.

Amherst College, Sept. 29, 1866.

5. *Laurite, a new mineral.*—Wöhler has discovered among the fine-grained platinum ore from Borneo, a new mineral, a sulphid of ruthenium and osmium, to which he has given the name *laurite.* It occurs in small grains of a dark iron-black color, and high luster. Most of the grains are true crystals, and Sartorius von Waltershausen has recognized the mineral to have the form of the regular octahedron, in some instances showing cubic, tetrahexahedral, and other planes. It has a distinct octahedral cleavage, is brittle and yields a dark gray powder on pulverization. Hardness, above that of quartz. Specific gravity, above 6 (6·99, Sartorius). When heated it decrepitates, and B.B. is infusible, giving first sulphurous and finally osmic acid fumes. Not acted upon by aqua regia or by fusion with bisulphate of potash. Fused with hydrate of potash in a silver crucible the mineral dissolves, yielding a green mass on cooling. Analysis gave, ruthenium 65·18, osmium 3·03, sulphur 31·79. The osmium was determined by loss, and Wöhler observes that the ruthenium was not entirely free from this substance, so that the percentage of ruthenium is given somewhat too high, while that of the osmium is correspondingly too low. The amount used for analysis (0·3145 gram), prevented a more accurate determination. Assuming the laurite to contain a sulphid of osmium analogous to osmic acid, the composition of the mineral may be represented by the formula

$$12(Ru_2S_3)+OsS_4=Ru\ 62{\cdot}88,\ Os\ 5{\cdot}00,\ S\ 32{\cdot}12,$$

or Ru_2S_3 91·8, OsS_4 8·2. This is the first instance of the occurrence of a natural sulphid in the group of platinum metals.—*Ann. Chem. Pharm.*, cxxxix, 116.

6. *Mt. Hood.*—The newspapers from the Pacific states contain accounts of two ascents of Mt. Hood during the past summer. From these, we glean but little information however, further than that the summit is accessible, for the accounts are very conflicting in their details, and some of the statements evidently very loosely made, while others are apparently wrong, which we must regret as the alleged facts may find their way into more enduring literature than the newspapers.

In July, 1864, the *Dalles* (Oregon) *Mountaineer* gave an account of a successful ascent made on the 17th of July of that year. The attempt was made by a party of four gentlemen of that place, three of whom gave out when near the summit, "but the fourth, Edward Ayres, persevered and succeeded in reaching the topmost pinnacle," which he represents as a "bare, rugged crag only large enough to stand upon." They found "a crater about 3000 ft. below the top, from which a sulphurous smoke ascended."

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[3] The distinction between this mineral and the clinochlore of this locality is perfectly easy. The latter scratches gypsum with facility, whereas the former makes no impression on it.

The present year, we have more detailed accounts of two other ascents. The first of these was made on the 26th of July, when "a party from Portland reached the summit after six hours travelling from the snow line." One of this party, Rev. H. K. Hines, gave an account in the Vancouver Register, of which we have seen only published extracts. He also speaks of "blue sulphurous smoke rising from fissures in the snow," and says that they "descended into the crater for a short distance but were stopped by a perpendicular wall fifty or sixty feet high." He describes the view as being especially grand, extending "from Mt. Rainer to Diamond Peak, a distance of 400 miles." (According to the maps, the actual distance between these peaks is but 260 miles.)

On the 20th of last August, another ascent was made by six gentlemen, one of whom, Prof. Alphonso Wood, has given a detailed account of the trip before the California Academy of Natural Sciences at their meeting of Sept. 3d. The report states that he found true glaciers on the flanks, with terminal and lateral moraines. He speaks of the slope having an angle of 45° for a mile and a half before reaching the crater, and 60° above that. The summit area he describes as "a crescent in shape, half a mile in length and three to fifty feet in width. It is a fearful place, as it is the imminent brow of a precipice on the north, sheer down not less than a vertical mile of bare, columnar rock." He too speaks of a crater, and we infer that it is very large and deep and mostly filled with snow except where melted by hot gases and steam. "On the west side of the crater is still an open abyss whence issue constantly volumes of strongly sulphurous smoke. That there is heat there, is evident from the immense depression in the snow about the place,—depressed not less than a thousand feet below the snows which fill to the brim the other portions of the ancient crater." He measured the various altitudes by observing the boiling point of water, and gives the following figures: "Summit of the Cascade Range and foot of Mt. Hood proper, 4,400 ft.; the limits of forest trees 9,000 ft.; highest limits of vegetation 11,000 ft.; summit of the mountain, 17,600 ft." The observed temperature of the boiling point he states at 180° F. on the summit, and from that deduces the last figures quoted, and closes by stating that we must consider this the highest measured point in the United States, if not in North America.

It seems highly probable however, that either his observations or his figures are very imperfect, and we are left still in the same uncertainty about the actual height of the peak that has existed heretofore. It is well known that the *estimated* height is stated in various works at from 7,710 ft. to 18,360 ft.

Those persons whose estimates seem entitled to consideration, (Lieut. Abbott, Dr. Newberry and others), state its height as less than that of Mt. Shasta, while the rough trigonometrical measurement made in 1860 by Dr. Vansant, gave the height at less than 12,000 ft. It is not easy to see how Prof. Wood deduces his figures from the observations he gives. A boiling point of 180° F. represents a barometric pressure of 15·26 inches, which in that region, in the month of August, would probably represent an altitude of 18,350 feet or more. On Mt. Shasta, which is 300 miles farther south, the forest vegetation barely extends up 9,000 ft., the alleged height on Mt. Hood, where the alpine species are probably identical or similar.

It is noticeable how these accounts differ in other important particulars. One finds the summit a mere "pinnacle" only large enough to stand upon—another speaks of it as half a mile long. All agree that there is a crater, but one party finds it 3,000 ft. below the summit; we infer from the description of another that it is at the summit, or at least that a part of its rim forms the summit; one party descended into it a short distance, but finds a precipice fifty or sixty feet high; another speaks of a precipice of a mile vertical. The last observers find the crater nearly filled with snow, while but a few months ago the papers contained accounts of the mountain in active eruption. From all these we see we are still in doubt as to the actual height and condition of the peak; but since these ascents demonstrate that the summit is easily accessible, we hope soon to have more satisfactory observations. W. H. B.

7. *Alleged discovery of an ancient human skull in California.*—Accounts have recently been going the rounds of the press of the discovery of a human skull in or beneath certain volcanic deposits in California, which has attracted much attention from the various ages that have been assigned to it. The facts of the case, so far as they have reached us from authentic sources, are as follows. The skull in question is alleged to have been found at a depth of 153 feet, in a shaft sunk in the consolidated volcanic ash, known locally as "lava," near Angel's Camp, in Calavaras county. Five beds of this consolidated ash were passed through, separated by beds of gravel.

The skull was found by a miner, and it soon came into the hands of Prof. J. D. Whitney, state geologist of California, who visited the locality and investigated the matter as far as was then possible, but owing to the presence of water and the stoppage of work in the shaft, the examination was not fully satisfactory. He has made a preliminary statement before the California Academy of Natural Sciences, but defers any extended notice until the subject can be investigated with more completeness and accuracy. He thinks the skull was found in the position claimed, and will investigate the subject when the water is pumped out of the shaft and work resumed, which is expected to be done soon.

The precise age of the beds in question is as yet uncertain. In the geology of California, Prof. W. considers that the eruption of the great mass of volcanic materials on the western slope of the Sierra Nevada began in the Pliocene age, and that it continued into the Post-pliocene, and possibly to comparatively modern times. The alleged position of the skull is a lower one than any in which the remains of the mastodon have there been found, and therefore the question of its authenticity becomes a very important one; and when the more complete examination has been made, we will lay the results before the readers of the Journal. W. H. B.

8. *On the discovery of the remains of a gigantic Dinosaur in the Cretaceous of New Jersey;* by E. D. Cope. (Proc. Acad. Nat. Sci. Philad., 1866, 275.)—Prof. Cope exibited the remains of a gigantic extinct Dinosaur, from the Cretaceous Green Sand of New Jersey. The bones were portions of the under jaw with teeth, portions of the scapular arch, including supposed clavicles, two humeri, left femur, and right tibia and fibula, with numerous phalanges, lumbar, sacral and caudal vertebræ, and numerous other elements in a fragmentary condition.

The animal was found by the workmen under the direction of J. C. Voorhees, superintendent of the West Jersey Marl Company's pits, about two miles south of Barnesboro, Gloucester Co., N. J.

The bones were taken from about twenty feet below the surface, in the top of the "chocolate" bed, which immediately underlies the green stratum which is of such value as a manure.

The discovery of this animal fills a hiatus in the Cretaceous fauna, revealing the carnivorous enemy of the great herbivorous Hadrosaurus, as the Dinodon was related to the Trachodon of the Nebraska beds; and the Megalosaurus to the Iguanodon of the European Wealden and Oolite.

In size this creature equalled the Megalosaurus Bucklandii, and with it and Dinodon, constituted the most formidable type of rapacious terrestrial vertebrates of which we have any knowledge. In its dentition and huge prehensile claws it resembled closely Megalosaurus; but the femur, resembling in its proximal regions more nearly that of the Iguanodon, indicated the probable existence of other equally important differences, and its pertaining to another genus. For this and the species the name of *Lœlaps aquilunguis* was proposed.

The paper continues with descriptions of the mandible, femur, tibia, fibula, humerus, phalanges, vertebræ, etc.

9. *Exploration of the "Bad Lands" or "Mauvaises Terres" of the Upper Missouri region;* by Dr. F. V. Hayden.—Dr. Hayden has just returned from an extended exploring tour through the region of the Bad Lands. On his way out he left Fort Randall on the Missouri river, August 3d, with an escort of five soldiers, a six-mule team, one assistant, a guide and Indian interpreter, and an Indian as hunter—nine in all. The party went up the Niobrara, north side, as far as Rapid river, passed up that stream to its head, crossed over the divide to the south fork of White river, passed along the south side of White river to White Earth creek, about 100 miles north of Fort Laramie, at which point they were nearly south of the Bad Lands. From thence they traversed the whole of the Bad Lands and returned on the old Fort Pine road, thence on the south side of the Missouri to Fort Randall, having been absent fifty-two days. Dr. Hayden has made very extensive collections of fossils, including about fifty turtles, two of them of the largest size, nearly or quite perfect. The distance travelled on the way out from Fort Randall and back was 650 miles, and the specimens obtained were transported by land through that wild country for more than 300 miles. We hope soon to give a full account of the results of the exploration, which must be of great importance to geological science, coming from one so able and experienced in exploration, and so familiar with the whole Upper Missouri region.

10. *Post-Tertiary of Maine.*—Mr. John De Laski has recently sent us a number of species of Post-tertiary fossil shells from the island of North Haven, Penobscot bay (the next island above Vinalhaven), Maine. The collections were made at a height of 350 feet by barometer above the sea; they were from a layer about three feet below the surface in a bog. Among the species there are the following: *Mytilus edulis*, *Pecten Islandicus*, *Mya truncata*, *Mya arenaria*, *Saxicava arctica*, *Serripes Grœnlandicus*, *Astarte striata*, *Natica clausa*, *Buccinum undulatum* Mœl., *Buccinum flectrum* Stimp., var. *Packardi*, *Fusus tornatus* Gould.

The *Buccinum flectrum*, var. was found with other shells by Abraham Carver in digging a well about two miles farther north, at a depth of sixteen feet, in blue clay beneath gravel and sand. The assemblage of shells is like that inhabiting the Banks of Newfoundland and coast of Labrador.—Eds.

11. *Discovery of Mastodon remains at Cohoes, N. Y.* (In a letter to Prof. Dana from Robert Safely, Esq., dated Cohoes, Sept. 27, 1866.)—During some recent excavations made by the Harmony Mills Co., Cohoes, (about 1,000 feet below Cohoes Falls) for the foundation of a new mill, a number of pot-holes were discovered in an ancient bed of the Mohawk river, one of which contained the lower jaw of a Mastodon imbedded in peat and drift-wood. These "pot-holes" are worn in the Hudson river shale, about a hundred feet above the present bed of the Mohawk, and about a mile from where it enters the Hudson. The one containing the remains was about 250 feet from the south band of the Mohawk. The jaw, which was in an excellent state of preservation, measured about 28 inches in length and 22 in breadth between the condyles. On the right side there was one molar, and on the left side two, one of which was 4 inches, and the other 6½ inches in length.

12. *An addition to some notes "On a few of the fossiliferous localities of Livingston and Genesee counties, N. Y.," published in the January No. of this Journal;* by Henry A. Green.—In the black shales of the Portage Group, at the Buck Run locality in this town, in which I have heretofore found only a few fossil plants, I have recently discovered several bones, probably fish. The exposed portion of one of these measures four by five inches, and appearances indicate that when worked out it will not be less than seven inches across. Others appear to be as large in at least one direction, and several of them are an inch or more thick. One of the larger bones is somewhat broken; otherwise they appear to be well preserved, and when worked out will probably be of considerable interest.

At Batavia I have recently found another outcrop of the Marcellus limestone. The character of the rock is such that the fossils can be got out much easier and better than at Avon. This outcrop will, I think, furnish some fossils not heretofore found in this limestone.

Mt. Morris, Aug. 29th, 1866.

III. BOTANY AND ZOOLOGY.

1. DECANDOLLE, *Prodromus, Syst. Nat. Regni Vegetabilis.* Pars XV, sectio posterior, *sistens Euphorbiaceas.* Paris, 1862 and 1866, pp. 1286.—A part of this thick volume, p. 1–188, containing *Euphorbia* and its near allies, elaborated by Boissier, was issued four years ago. The rest of the *Euphorbiaceæ*, very ably worked out by J. Müller, is now published under the date of August last. The extent of the order has evidently confounded the calculations of the editor; for this thick volume of almost 1300 pages, occupied by the *Euphorbiaceæ*, does not comprise all that was originally assigned to it, the *Bruxaceæ*, cited as synonymous on p. 1, being now excluded and referred to the ensuing volume. Under Dr. Müller's hands, the genera are arranged upon an intelligible system, under ten neatly characterized tribes, and the genera are not a little reduced. In consequence, *Phyllanthus*, with 438 species almost rivals *Euphorbia* itself; and *Croton*, received also almost in the widest sense, displays 453 species. *Acalypha*, the character of which remains unchanged, is here augmented to 215 species. It is a satisfaction to find, and it is a probable indication of superior excellence in this elaboration, that the number of species in relation to genera is unusually high, and that there are very few genera founded on single species. In view of facility of reference it is greatly to be desired,—and perhaps it is still not impossible by the aid of new title pages,—that these volumes 15 and 16 should be re-numbered and conformed to the actual state of the case; this huge second part of vol. 15 to be 16, the present 16, of which one fascicle has been issued, to be 17, and so on. The permanent advantage would much exceed any temporary inconvenience of the change. A. G.

2. E. BOISSIER, *Icones Euphorbiarum, ou Figures de* 122 *Espèces du Genre Euphorbia, dessinèes et gravèes par* HEYLAND, etc. Paris, Victor Masson et fils. 1866. Royal fol.—Along with the volume of the Prodromus devoted to *Euphorbiaceæ*, we opportunely receive M. Boissier's magnificent folio, in which he has illustrated 122 selected species of the vast genus *Euphorbia*, one species to each plate. The plates, it will be seen are put upon stone, as well as drawn, by the veteran artist Heyland, and are in his best style,—a large part in outline, which is well adapted to this subject. Botanists are deeply indebted to M. Boissier for bringing out—evidently at much expense—this important work. A fair share of North American species are figured, viz: *E. acuta, angusta, zygophylloides, revoluta, florida, glyptosperma, hexagona, bilobata, exstipulata, Mercurialana, Wrightii, sphærosperma, trichotoma, dictyosperma, Texana, Peplidion, Rœmeriana,*—most of them species recently established either by Boissier or by Dr. Engelmann. Several pages of letter-press are occupied with remarks on the structure, classification, and geographical distribution of the genus. Linnæus described 64 species of *Euphorbia;* Boissier, in the Prodromus, including the recent supplement, has 717. A. G.

3. *On the young stages of a few Annelids;* by ALEXANDER AGASSIZ. (Extracted from the Annals of the Lyceum of Natural History of New York, vol. viii, June, 1866.)—In this paper, which is prefaced by useful

observations on the habits and modes of collecting the young of these and other marine animals, the author has presented many new and valuable contributions to our knowledge of the development of several species of Annelids. Among these are species of *Planaria*, *Spirorbis*, *Terebella*, *Polydora*, *Nerine*, *Phyllodoce*, and *Nareda* (?). The latter is compared with the unknown larva described by Loven in 1842, which is, therefore, supposed to belong to some Nemertean genus like *Polia*. The paper concludes with brief remarks upon the Types of Development in Annelids. The work is illustrated by six wood-cut plates, containing fifty-six figures. A. E. V.

4. *Corals and Polyps of the North Pacific Exploring Expedition, with Descriptions of other Pacific Ocean species*, with four plates; by A. E. VERRILL. (Extracted from Proceedings of the Essex Institute, vols. iv and v.)—This pamphlet has been published in three parts, of which the two first have already been noticed in this Journal. Part III contains the *Madreporaria*, illustrated by two plates, one of which includes also a few species of *Actinidæ*. Three new genera, *Pachysammia*, *Cœlastrea* and *Cyclopora*, are described, and thirty-seven new species. Among the more interesting forms are a living *Eupsammia*, hitherto a tertiary genus, three species of *Stephanoseris*, an *Allopora* from California, and a *Diaseris*, with a figure of the living polyp.

5. *On the Polyps and Corals of Panama, with descriptions of new species;* by A. E. VERRILL. (From the Proceedings of the Boston Society of Natural History, April, 1866.)—This paper is prefaced by a comparison of the Polyp faunæ of the Atlantic and Pacific shores of Central America, showing a remarkable contrast—as had been previously determined for the Mollusca, Crustacea, and other classes, giving additional evidence of the improbability of oceanic communication across the Isthmus in recent geological times. Four new species of Alcyonaria and eight of Madreporaria are described; also a new genus, *Stephanocora*, belonging to the *Poritidæ*. All of the previously described species are mentioned, with their known distribution, and other observations.

6. *On the Polyps and Echinoderms of New England, with descriptions of new species;* by A. E. VERRILL. (Published and stitched with the preceding.)—In this paper special attention is devoted to the geographical distribution of these two classes on our coast, which is discussed in the introduction. The New England coast is considered as embracing a part of three marine faunæ: the Virginian south of Cape Cod, the Acadian along the eastern coast, and detached patches of the sub-arctic or Syrtensian fauna on deep-lying banks (like St. George's) off the coast. The theory is advanced that "an increase in *depth* of water has the same effect as increase in the *elevation* of land—that of causing a lower temperature, and consequently bringing northern animals down to lower latitudes than they can inhabit in shallower waters along the shore, thus giving rise to outlying patches of more northern faunæ far south of their proper limits on the coast."

The paper embraces a complete list, so far as known, of all the species in each fauna, with remarks on their distribution, their synonyms, &c. Two new species of *Sagartia* from near New Haven, and the two large species of *Asterias* from the eastern coast, not before sufficiently characterized, are described. A new generic name, *Euryechinus*, is proposed for our common Sea-urchins, of which two species are recognized. A new genus, *Leptasterias*, is established for the small starfishes like *A. Mulleri* Sars, and a new species is described. For the *Psolus Fabricii* Lutken, a new genus, *Lophothuria*, is instituted.

This being the first attempt yet made to bring together and revise the synonymy of all our species of Echinoderms, it will doubtless be found very useful to those interested in the subject.

7. *Natural History of Animals;* by Prof. SANBORN TENNEY and Mrs. ABBY A. TENNEY. New York, 1866. (Chas. Scribner & Co.)—This little work is intended for beginners in natural history, and contains brief descriptions of the animals figured on the natural history tablets designed to accompany it. The book, however, is complete in itself, and contains five hundred beautifully executed wood-cuts, being a reprint of those in Tenney's Manual of Zoölogy. The attempt has been made in this book to free the subject from all technicalities, and to simplify it to the utmost extent. It would have been more generally useful, however, had the scientific names of the animals described or figured been given as well as the common names. Affording, as it does, figures and brief descriptions of large numbers of American animals, as well as some foreign, in a neat book at a low price, while it gives a general view of the animal kingdom in a popular form, it cannot fail to meet a want felt by many, for elementary works on American Natural History. A. E. V.

8. *Note on the Organisms of the Geysers of California;* by Prof. W. H. BREWER.—In the May number of this Journal (p. 392), in a letter to Prof. J. D. Dana, on the presence of living species in hot and saline waters of California, I misapprehended certain facts, relating to organisms in the hot waters of the geysers. I there stated that Mr. A. M. Edwards of New York had detected "animal as well as vegetable organisms in the specimens." Mr. Edwards writes me that he examined specimens collected "over hot stoves in water at 120·5° F.," and he states that he found a few remains of *Diatomaceæ*, of which he enumerated several species. No animal remains were found but such fragments (hairs) as might have been derived from outside sources. It is due Mr. Edwards that I make this correction, which arose from a misapprehension on my part of what species had been detected. In regard to the existence of other vegetable forms in waters of a higher temperature (200° F.), observed by myself, the facts were correctly given.

New Haven, Oct. 19, 1866.

IV. ASTRONOMY.

1. *Shooting Stars in August*, 1866.—(1.) *At Sherburne, N. Y.*—On the morning of the 10th of August the writer, with three assistants, saw *seventy-six* shooting stars between 12 and 1 o'clock. The floating clouds interfered seriously for a part of the hour. During the next hour we saw *thirty-eight*, notwithstanding the clouds, which were always numerous, and at times covered the sky. On the next morning the sky was clear, and watching alone I saw 51 shooting stars in the hour between one and two o'clock. The number seemed to diminish toward morning.

(2.) *At Germantown, Pa.*—Mr. B. V. Marsh and Mr. R. M. Gummere watched with the following results.

Aug. 9th, between 8^h 40^m and 9^h P.M. Mr. Marsh alone saw 7 meteors, 5 conformable. Aug. 10th, between 8^h 47^m and 9^h 7^m P. M., 10 meteors. The weather was very fine. There was perhaps a slight haziness, but very small stars were very distinct. The following table exhibits the results of observation on the morning of the 11th.

	By Mr. B. V. Marsh.			By Mr. R. M. Grummere.		
	Conformable.	Non-conformable.	Total.	Conformable.	Non-conformable.	Total.
h m h m						
From 0 0 to 0 15 A.M.	14	1	15			22
0 15 " 0 30 "	15	3	18			19
0 30 " 0 45 "	8	2	10	6	2	8
0 45 " 1 0 "	13	3	16	24	3	27
1 0 " 1 15 "	14	4	18	13	3	16
1 15 " 1 30 "	8	5	13	13	4	17
1 30 " 1 45 "	10	3	13	15	3	18
1 45 " 2 0 "	11	3	14	19	2	21
2 0 " 2 15 "	10	2	12	16	0	16
	103	26	129			164
	Average, 57·3 meteors per hour.			Average, 72·9 meteors per hour.		

The average magnitude was decidedly below that of former years. Only a few left persistent trains, and there were none of very great splendor. The weather was clear and circumstances were altogether favorable. The two observers were independent of each other.

(3.) *At Westchester, Pa.*—J. H. Worrall, Ph.D., watched on two mornings with these results:

	Aug. 11.	Aug. 12.
From 2^h to 2½^h A. M.	25 meteors.	16
" 2½ to 3 "	18 "	15
" 3 to 3½ "	19 "	21
" 3½ to 4 "	13 "	11
Total,	75	63

The sky was perfectly clear from all haze on the first night. The clouds on the second night obscured the south and southeast to 30° high. The star ε Ursæ Minoris was distinctly visible. During both nights the meteors emanated from a point near the constellation Perseus. They were irregular in the intervals of their appearance. Sometimes several would follow in quick succession, and then several minutes would elapse without one being seen.

(4.) *At Natick, Mass.*—Mr. F. W. Russell, on the evening of Aug. 6th, saw in 1½ hours, 10 meteors (2 conf. and 8 unconf.); on Aug. 7th in 1 hour, 20 meteors (7 conf. and 13 unconf.); and Aug. 8th in one hour, 24 meteors (14 conf. and 10 unconf.). It rained on the 9th. On the night of the 10th–11th he saw as follows, watching alone:

9^h–10^h	16 conf.	8 unconf.	Total 24	
10–11	24 "	10 "	" 34	
11–12	44 "	11 "	" 55	With trains, 13
12– 1	59 "	9 "	" 68	" 13
1– 2	76 "	10 "	" 86	" 10
2– 3	86 "	11 "	" 97	" 21
Total in 6 hours,	305	59	364	57

(5.) *At sea near Martha's Vineyard.*—Mr. Isaac Pierson saw, while entering Martha's Vineyard Sound, 100 meteors in two hours between 9^h 15^m and 11^h 30^m P. M. of Friday the 10th of August—omitting the quarter hour from 10^h 45^m to 11^h. During the first half hour the sky was about one-eighth covered with clouds.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *National Academy of Sciences.*—The sixth stated session of the National Academy of Sciences was held at Northampton, Mass., on the 7th of August last. The following is a list of the papers read:

(1.) On a photometric method, by Prof. O. N. ROOD.

(2.) On a normal map of the solar spectrum, by Prof. WOLCOTT GIBBS.

(3.) On traces of glaciers under the tropics, by Prof. LOUIS AGASSIZ.

(4.) On the secular acceleration of the moon's mean motion, by JOHN N. STOCKWELL; read by Dr. B. A. Gould.

(5.) On the origin of solar heat, by Prof. B. PEIRCE.

(6.) On the morphological value and relations of the human hand, by Dr. BURT G. WILDER.

(7.) On the correlation of gravity and temperature, by PLINY E. CHASE.

(8.) On the grounds of analogy between linguistic science and the physical sciences, by Prof. WM. D. WHITNEY.

(9.) On the limitation of homologies, by Prof. LOUIS AGASSIZ.

(10.) On a new method of optical analysis, by Prof. WOLCOTT GIBBS.

(11.) On recent soundings in the Gulf Stream, by Mr. HENRY MITCHELL, U. S. Coast Survey.

(12.) On repeated linear substitutions, by J. E. OLIVER.

(13.) On the metrical system of weights and measures, by SAMUEL B. RUGGLES.

(14.) On some points in the geological structure of southern Minnesota, with reference also to the period of denudation of the older formations, by Prof. JAMES HALL.

(15.) A new theory of planetary motion, by Prof. T. STRONG.

(16.) On the linear evaluation of surd forms, by Prof. JAMES WATSON.

(17.) On the study of young animals, and its bearing upon the progress of paleontology and zoölogy, by ALEX. AGASSIZ; read by Prof. Louis Agassiz.

(18.) On a remarkable rainbow, by Prof. PEIRCE.

(19.) An investigation in regard to sound in its economical application, by Prof. JOSEPH HENRY.

(20.) On the geographical distribution of fishes in the waters of the Amazon, by Prof. LOUIS AGASSIZ.

(21.) On the stature of American soldiers, by Dr. B. A. GOULD.

(22.) On the influence of the hour of the day on the height obtained by barometric measurements, by Prof. A. GUYOT.

(23.) On astronomical photography, by LEWIS M. RUTHERFURD.

(24.) On the reduction of photographic observations, with a determination of the position of the Pleiades from photographs by Mr. Rutherfurd, by Dr. B. A. GOULD.

(25.) On a table for facilitating the conversion of longitude and latitude into right ascension and declination, by WM. FERREL.

(26.) On the Nephila plumipes or silk spider of South Carolina, by Dr. Burt G. Wilder.

Prof. J. P. Lesley read a biographical notice of the late Prof. Edward Hitchcock.

2. *Meteorite in Hungary of June,* 1866; by Joseph Szabo, Professor of Mineralogy and Geology at the University of Pest, and Reporter of the Committee for mathematical and physical sciences at the Academy. (From a letter to Prof. Henry, Smithsonian Institution, dated Pest, July 31, 1866.)—A fall of a meteorite took place in the N.E of Hungary, the 9th of June 1866 at 5 P. M. The locality is a small village called *Knyahinya*, inhabited by Rusznyaks, about one Austrian mile from the village Gr. Berezna, and five miles from Ungvár, the capitol of the county of Ung; the place above named is on the boundary of the two counties Ung, and Zemplin, and N.N.W. from Ungvár. We call it therefore "the meteorite from *Knyahinya* N.W. of Hungary."

The luminous meteor was first observed in the neighborhood of Kaschan, and was seen to proceed in an eastern direction. The people nearer to the east say that there was a violent detonation, accompanied by a small cloud, and by the fall of several pieces of stone. A Jew, not far from whom a piece fell, after having taken it into his hands, felt it *cold.*

About seventy pieces have been found; the largest is said to have a weight of 36 Austrian pounds, but it was broken on the spot by the finders, so that the largest piece of it weighs now about fourteen pounds. It became the property of a naturalist in Pest. I have received a notice of three pieces, weighing each about 200 Vienna pounds; but this fact must be verified.

The Hungarian Academy of Sciences at Pest has taken proper measures in order to save a part of the fallen pieces for the interest of science, and it has become, through the coöperation of disinterested and zealous persons, the proprietor of nearly half of them. It has been determined to send some of them to different scientific institutions in Europe and in the United States, and thus also to the mineralogical department entrusted to your care. I have the honor to announce to you, that a piece in a perfect state of preservation will be transmitted to you about the month of September, after the examination of some of its physical properties is completed. Dr. Than, professor of chemistry at the university, is occupied in making a chemical analysis of it.

3. *Lyceum of Natural History, New York.*—We mentioned in the July number of this Journal, the destruction by fire of the building of the Lyceum of Natural History of New York. We are informed that, although the Lyceum suffered great and, in some respects, irreparable loss, in the destruction, for the most part, of the zoological, mineralogical and paleontological portions of its collection, yet its very extensive and valuable herbarium—(containing plants collected and determined by such eminent botanists as Torrey, Gray, Halsey, the two Carys, Bulkley, Browne and others) was preserved intact. So also was its Library, comprising series of the Transactions of nearly all the learned societies of Europe and America, and the stock of the Lyceum's own "Annals." All these had been removed from the Medical College a year or two previous to the fire,—and by the kindness of the Directors of the Mercantile Library Association, had been permitted to occupy a place upon its shelves.

To repair, as far as possible, the loss which the Lyceum has sustained, many of its members stand ready to give large private collections in the several departments of natural history, whenever a sufficient and safe building shall be secured for their reception. For this, they look to the known liberality and public spirit of the wealthy citizens of New York, and believe they will not look in vain.

4. *Gifts of Mr. George Peabody to Science.*—Mr. George Peabody has recently given $150,000 to Harvard College for the establishment of a Museum of American Archæology and Ethnology, and the same amount to Yale College for a Museum of Natural History.

5. *British Association.*—The meeting of the British Association for the current year was held at Nottingham during the week commencing Aug. 22d, Mr. Groves being the President. Dundee was appointed as the place of meeting for the next year.

6. *Dr. Krantz.*—Dr. Krantz has purchased the extensive collection of fossils and minerals belonging to the Comptoir Minéralogique and Géologique of the late Mr. Louis Sæmann of Paris, and is about adding them to his own great establishment for the sale of specimens at Bonn on the Rhine.

OBITUARY.

Mr. Edmund Blunt, first Assistant upon the United States Coast Survey, died on the 2d of September last. Mr. Blunt was a son of Edmund M. Blunt, author of the American Coast Pilot, and was born in Newburyport in Nov., 1799. In early life he manifested a great interest in hydrographical pursuits. In 1816 he made a survey of the harbor of New York, and this was the first survey of that harbor ever made, for the chart of Des Barras must be considered simply as a sketch. In 1819 and 1820 he made the first surveys of the Bahama Banks, and the shoals of George's and Nantucket. In 1824 he surveyed the entrance of New York harbor from Barnegat to Fire Island. In 1825 and 1826 he run a line of levels from the river St. Juan to the Pacific Ocean for the purpose of building a canal on the Nicaragua route. From 1827 to 1830, as a private enterprise, he surveyed Long Island Sound from New York to Montauk Point, the government up to that time having done nothing whatever to develop a knowledge of the coast of the United States. On the organization of the U. S. Coast Survey, Mr. Blunt was appointed first assistant, which position he held until his death.

He triangulated Long Island, the shores of Connecticut and Rhode Island, ending with a base of verification near Providence, R. I. He also triangulated Delaware Bay from Philadelphia to the capes; also the Chesapeake from its head to the capes; and the Hudson river to a point above Troy. In 1855 and 1856 he furnished the points to determine the exterior line of New York harbor, which has done so much for its preservation.

Before and while on the Coast Survey, his attention was directed to the inferiority of the lights in the American light-houses, and he was mainly instrumental in introducing the Fresnel system into our country; a system which has contributed so much to the safety of navigation. Mr. Blunt was a mechanic of great inventive power. The dividing engine, built from his plan and under his direction, is an evidence of his knowledge. Mr. Blunt was a true American, always solicitous for the honor and advancement of his country; and when the late rebellion broke out, he devoted all his energies to the support of the government.

A. A. GOULD.—Dr. Augustus A. Gould died in Boston, Sept 15th, at 5^h 30^m A. M., of cholera, after an illness of only a few hours. Dr. Gould was the son of Deacon N. D. Gould, late of Boston. He was born in New Ipswich, N. H., April 23, 1805, and graduated at Harvard College in 1825. He pursued the study of medicine with Drs. James Jackson and Walter Channing, and immediately thereafter commenced practice in Boston. Although constantly engaged in the active duties of his profession, science was the leading passion of his life, and by zealously devoting his leisure moments in the intervals of business, and, as he has expressed it, "hours stolen from sleep" to his favorite studies he has made his name widely known as a scientific student and author by many valuable contributions. He became very early one of the most active members of the Boston Society of Natural History, and has continued his interest and coöperation in it until the day of his death, holding then the office of vice-president, a position he has filled for several years. The day before his death he spent a long time at the Society rooms, probably the last business that he did away from home. He was also a Fellow of the American Academy of Arts and Sciences; of the American Philosophical Society; of the National Academy of Science; and two years ago was unanimously elected President of the Massachusetts Medical Society. Many of his contributions to science have been published in the Proceedings and Memoirs of these societies. Many of his conchological papers, especially, have appeared in the Journal and Proceedings of the Boston Society of Natural History. In 1841 he published his Report on the Invertebrates of Massachusetts, an appropriation for that purpose having been made by the State. This, being one of the pioneer works on the subject in this country, is remarkable for its accuracy and general usefulness, and has always been one of the standard works on American conchology, that part of the book relating to the shells being the most voluminous and complete, and each species being well figured from drawings made mostly by the author's hand. The Legislature of 1865 made an appropriation of $4,000 to republish this work, and for several months he has been engaged in revising and enlarging it for that purpose. It is to be hoped that this labor was so nearly completed as to admit of the issue of the work at an early day. He published in connection with Prof. Agassiz, the "Principles of Zoölogy," in 1848. This work has become well known and widely circulated. In 1846 he was employed by the United States government to write the Report upon the Shells of the Wilkes Exploring Expedition, and contributed a quarto volume, with a folio atlas of plates, toward the history of that voyage. In 1863 he published, under the title of "Otia Conchologica," all the original descriptions of new species of shells published in his various works, with notes on changes in their nomenclature. His extensive collection of shells was recently purchased by the Boston Society.

His contributions to medical science are also numerous. In the department of vital statistics he was eminent among American students of that subject. He contributed to nearly every volume of the Registrar-General of Massachusetts papers of great labor and value.

He was a man of wide and general culture, and stood in the highest rank of professional eminence. His influence in promoting and diffusing the study of Natural History will never cease to be felt in America.

R. W. GIBBES.—Robert Wilson Gibbes, of Columbia, South Carolina, died in that city near the close of September. Dr. Gibbes had been one of the most active men of science in the Southern States. He was born in Charleston, S. C., July 8th, 1809. His chief scientific researches were directed toward the description of organic remains from his native State, and his memoirs include a "Monograph on the fossil Squalidæ of the United States;" a "Memoir on the fossil genus Basilosaurus," and another on "Mosasaurus and the three allied new genera, Holocodus, Conosaurus, and Amphorosteus," the first two published in the Journal of the Academy of Sciences of Philadelphia, and the last in the Smithsonian Contributions to Knowledge, vol. vii, Nov. 1849. He was also the author of important papers on medical subjects, and of a "Documentary History of the American Revolution," in three volumes. His house, with its library and collections, was destroyed during the passage of Gen. Sherman's army through South Carolina near the close of the late rebellion, which he made the reason for his formal withdrawal from the American Association for the Advancement of Science at their late session in Buffalo.

LOUIS SÆMANN.—Mr. Sæmann died suddenly at Paris on the 23d of August last. He had been for many years proprietor of a large establishment in that city for the sale of minerals and fossils, and by his fidelity in all business transactions and the urbanity of his manners had won the confidence and regard of all who came into contact with him. He also commanded their respect as a man of science, for he was an excellent mineralogist, geologist, and paleontologist, and had published valuable papers in each of these departments. After extensive tours in Europe he came to this country in 1847 and spent nearly a year in making extensive collections in geology and mineralogy. Prof. Dana acknowledges his indebtedness to Mr. Sæmann in the Preface of the last edition of his Mineralogy for the facts and suggestions which he had communicated; and in the preparation of the new edition, now in progress, his correspondence has been of like service and value.

Major ROBERT KENNICUTT.—It is with great regret that we have to announce the death of Major Kennicutt, chief of explorations of the Russian-American Telegraph Co. The news came first by telegraph. The following brief account of the circumstances of the event we gather from a letter from Mr. Charles Pease, one of the party under his command, to his friends at Cleveland, Ohio. Major Kennicutt, with a corps of naturalists, under the patronage of the Overland Telegraph Co., had made his way by land from San Francisco to the North Pacific. After their arrival at St. Michael's he met with many disappointments and failures, and though they were perhaps unavoidable, the effect upon him was very disastrous. They seemed to overcome him more than the hardships and sufferings he had previously undergone. He complained much of dizziness and strange distress in his head. On the morning of the 13th of May he was found by two of the party not more than two hundred yards from the Russian fort Nulato, lying upon the ground, dead. An open compass lay beside him; apparently he had been taking bearings and had

fallen suddenly. With very great difficulty the remains were brought on by his friends to Fort St. Michael's, from which it is expected they will be transported to this country. The death of this young, enthusiastic and persevering explorer is a great loss to the cause of science. We trust in another number to give a sketch of his brief career.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Smithsonian Institution: Report of the Secretary to the Board of Regents, January*, 1866 (being a part of the annual volume for 1865). Washington, D. C., 1866. 8vo, pp. 74.—The Secretary gives in this Report his usual summary of the various and varied operations of the Smithsonian Institution for the year 1865. A review is given of the circumstances resulting in the destruction of a portion of the building and contents by fire in 1864. The fire had its origin in an unpardonable blunder of a workman who opened a communication for a stove-pipe into a "furring space," mistaking it for a chimney flue. The products of combustion were thus brought among the roof-timbers and ultimately set them on fire. Nothing could better illustrate the importance of making such buildings fire proof. The very roof which burned the building was made in wood (although covered with slates), from motives of economy, in the original construction. The loss of the manuscript correspondence from the office of the Secretary, of his private manuscripts and records of subjects of science, of memoirs accepted for publication, copies of the Smithsonian reports, stereotype plates, wood-cuts and steel plates, seven years records of the force and direction of the wind in the anemometer tower, are the chief sources of regret in the results of the fire, although the loss of the apparatus presented by Dr. Hare, of Priestley's lens with which the discovery of oxygen was made, and many interesting personal memorials of Smithson is irreparable. The plan adopted for the reconstruction of the portions of the building destroyed contemplates the use of only fire-proof materials. The outlay required to meet these expensive restorations will be so managed as not to impair the general operations of the institution, but will prevent the undertaking of new enterprises.

The current income of the Institution for 1865 was $84,956.37, of which $32,115.97 were expended for the maintainance of the establishment and for carrying on all the operations of the Institution, leaving the balance available for reconstruction. The fund has received an addition from the death of an annuitant of Smithson, which amounts to $54,165.38, invested in U. S. $7\frac{3}{10}$ per cent bonds, the increase of which will hereafter be available for the general fund.

As the government of the United States is largely debtor to the Smithsonian Institution for labor bestowed on its scientific work, and much the larger part of the building and its maintainance are for the conservation of the public collections, there can be no reasonable doubt that the cost of restoring the building ought to be paid out of the public treasury, including the arrearages already paid by the fund, thus restoring the full value of Smithson's bequest to its legitimate objects, "the increase and diffusion of knowledge among men." The jealous care and watchful integrity and courage with which the distinguished Secretary has ever guarded the main object of the trust, often against powerful official and legislative influences, is worthy of all praise, and receives the commendation of investigators of truth everywhere.

The volume also contains a number of valuable memoirs of considerable length either prepared for the Report, or cited from foreign Journals, including a paper on the Aurora Borealis, by ELIAS LOOMIS; on the Senses of Feeling and Smell; on Electro-physiology, by MATTEUCCI; on Lacustrine Constructions on Lake Neuchatel, with many figures, by E. DESOR; Systematic Review of the Class of Birds, by Prof. LILLJEBERG, etc.

2. *History of the Atlantic Telegraph;* by HENRY M. FIELD, D.D.—This work is a vivid sketch of the entire history of transatlantic telegraphy, by a brother of Mr. Cyrus W. Field, whose name has become forever identified with the inception and progress of that great enterprise which, after surmounting vast difficulties, has at last united Europe and America.

The new cable encloses and insulates a copper conductor, formed, like the old one, of seven strands of the purest copper, the weight of which is three hundred pounds to the mile; the old one weighed but one hundred and seven pounds. This conductor was first imbedded in "Chatterton's compound," a preparation impervous to water, and then covered with four layers of gutta percha, laid on alternately with four thin layers of Chatterton's compound. The old cable had but three coatings of gutta percha with nothing between. Its entire insulation weighed but two hundred and sixty-one pounds to the mile, while that of the new one weighed four hundred pounds. The armor which gave the requisite strength to the new cable consisted of ten solid wires of the best soft steel, each wire being surrounded separately with five strands of Manilla yarn saturated with a preservative compound. This armor was laid on spirally about the core, from which it was also separated by a padding of hemp saturated with the same preservative mixture. The cable thus constructed combined the flexibility of a ship's rope with the strength of an iron chain, while its greater volume gave it an important advantage in buoyancy over the old one, causing it to sink slowly and avoiding the danger of fracture from a sudden lurch of the ship, which in 1857 caused the parting of the cable of that year. The breaking strain of the new cable was seven tons and fifteen cwt.; that of the old one was three tons and five cwt. The contract strain of the new cable was eleven times its weight per mile in water, the greatest depth of water to be passed being but two and a half miles.

Capt. (now Sir James) Anderson, who was in command of the Great Eastern, is a navigator whose attainments in his profession peculiarly fitted him for the difficult task of laying the cable, and the yet more difficult one of recovering that of 1865 from its resting place in mid-ocean. The satisfactory discovery of perfect insulation in this cable after being a year submerged, is not the least noteworthy feature of the series of successes which has been rewarded with knighthood conferred on Anderson, Thompson, and their companions, by the Queen of England.

3. *Elements of Quaternions;* by the late Sir WILLIAM ROWAN HAMILTON; edited by his son, William Edwin Hamilton. 8vo, pp. lx, and 762. London. (Longmans, Green & Co.)—This volume had been promised and its appearance was awaited with interest by mathematicians for some

time previous to the lamented death of its distinguished author. It was not quite finished when he died, and the son has published it as left by the father. The Board of Trinity College, Dublin, has generously assumed the expense of the publication.

The first development of the Calculus of Quaternions was communicated in 1843 by Hamilton to the Royal Irish Academy. Ten years later a more complete and more geometrical presentation of this remarkable addition to Mathematical science was published in a thick octavo volume entitled *Lectures on Quaternions* (Dublin 1853). The later years of the author's life have been spent upon the present volume, which covers the same ground as the *lectures* and yet can hardly be regarded as a second edition of them.

In the hands of the author this Calculus had most wonderful scope and power. Whether it is to be the great instrument which future mathematicians will employ in subduing new realms of the Physical and exact Sciences, and in developing the riches of the old, it is probably premature to assert. Those capable of judging believe that its power and importance cannot be overestimated, and that it is destined to change the whole character of the higher mathematics.

A student should read this volume rather than any presentation of the subject that may be given by another mind. It requires a previous knowledge of the elementary mathematics including the more recent developments in Geometry as given in *Salmon's Conic Sections* or *Chasles' Geomètrie Superieure*, the Geometry of three dimensions, Differential and Integral Calculus, and the applications of the calculus to geometry. Some portions of the volume, especially the later pages, imply also a knowledge of Analytical and Celestial Mechanics.

The question is often asked, "What is a Quaternion?" A brief explanation may serve to the curious as a partial answer. We sometimes say that there are in geometry three kinds of magnitudes, volumes, surfaces, and lengths. The first are increased by the product of three linear factors, the second by the product of two linear factors, and the third by linear terms. If now we extend this series downward, we have a fourth class of magnitudes which are severally measured by the ratio of one line to another, that is, by a zero number of factors. Such are the circular functions, and angles. To this class belongs the Quaternion. Like the sine and the tangent it is the quotient of one line divided by another. But the lines are considered, in this instance, to have not only length but also direction in space. There enters into the conception of the quaternion, 1st, the relative length of the two lines; 2d, the angle which they make with each other; and 3d, the direction of their plane. The first two of these are each determined by a single condition, the third by two conditions. Hence there are four arbitrary quantities in any algebraic expression of this quotient, from which comes the name *quaternion*. The power of this calculus regarded as an extension of algebra, depends upon its use of the imaginary expressions to denote directions. H. A. N.

4. *A Preliminary Report of the Texas Geological Survey, together with Agricultural observations and an outline of the Mineral Deposits of the State;* by S. B. Buckley. 86 pp., 8vo. Austin, Texas, 1866.—Mr. Buckley was assistant to Dr. Moore, who had the charge of the geological survey of the state previous to 1861 when the survey was sus-

pended, and before that to Dr. B. F. Shumard. He presents in this report results of the observations made in the course of the preceding survey, results which show how much remains to be done before we have any just idea of the geology of the great state. The pamphlet consists largely of miscellaneous information on general geology, agricultural topics, and other matters of economical interest, together with an appendix containing descriptions of some Texan grasses regarded as new species.

5. *On the Geology of the Key of Sombrero, W. I.;* by ALEXIS A. JULIEN, Assistant in the School of Mines, Columbia College, New York. 28 pp. 8vo, with two plates. (From the Annals of the Lyc. of N. York, vol. viii.)—Mr. Julien has already published in this Journal on the minerals of the guano-bearing coral island Sombrero. This memoir gives a geological account of the structure of the coral island itself, and of the changes it has undergone. From the study of the several beds of coral rock and the surface features, he concludes that there were both subsidences and elevations in the progress of the island, and makes out eight elevations, and ten subsidences. The eighth elevation was to a height of 160 feet, after which there was a subsidence of about 40 feet, and another of 80 feet. The length of the island is about one mile; the breadth 200 to 1500 feet; the height over the southern third about 40 feet above the sea, and over the central and northern, half this. For the evidences of the changes of level and other points connected with the deposits of guano we refer to the memoir.

6. *Memoir on the Island of Navassa, W. I.;* by EUGENE GAUSSOIN, Mining Engineer and Metallurgist. 32 pp. 8vo. Baltimore, 1866. Also, *The Island of Navassa illustrated*—folio. Idem.—The island of Navassa is, like Sombrero, an elevated coral island, affording guano. Mr. Gaussoin has given an interesting description of this island, and illustrated the subject with six large folio chromo-lithographic plates, containing various views of the island, and making an elegant atlas. This island is in the Caribbean sea, in 18° 25′ N. and 75° 5′ W., 33 miles southwest of Hayti. The greatest height is 300 feet. It has perpendicular cliffs of compact coral-made limestone on all sides, and these cliffs are penetrated, as usual in such cases, by numerous caverns. The summit is lowest at center, atoll-like; and there is abundant evidence that the island is an elevated atoll, whose sides have been worn off into cliffs by the battering waves. Whether there were successive stages in the elevation remains to be ascertained. The change of limestone to phosphate through the presence of guano deposits is abundantly exemplified in many places on this island, as it is on Sombrero. The freshwater of the island the author remarks is simply the water of the rains which descends below the surface and rests on the subjacent saltwater; and he observes that in digging for water it is important not to go below lowtide level, as the water then becomes brackish. In this view he concurs with R. J. Nelson, the author of a memoir on the geology of the Bermudas. The volume closes with analyses of the guano of the island.

7. *Peat and its uses, as fertilizer and fuel;* by SAMUEL W. JOHNSON, Prof of Analyt. and Agricult. Chem., Yale College. 168 pp. 12mo. New York, 1866. (O. Judd & Co.)—This little manual contains more information upon the subject of which it treats than any other work with which

we are acquainted, and it is eminently practical in the arrangement and treatment of the topics embraced. It is divided into three parts, the first treating of the origin, varieties and chemical characters of peat; the second, of its agricultural uses, including muck; the third, of its uses for fuel.

Several years since the same author, as chemist to the State Agricultural Society of Connecticut made an extensive investigation into the chemical characters and agricultural uses of these substances, and his Report, published by the society, is well known. Since the publication of that report, additional investigations have been made, one series of which possess a peculiar interest to the agriculturist, as they relate especially to the action of various composting materials upon peat. Without reviewing here the details of these experiments, which are given at length in the work (p. 77), it is sufficient to state the following as among the results; that the admixture of ashes, carbonate of lime, slacked lime, and Peruvian guano, tended to greatly increase the amount of plant food in decomposing peat, the crops in extreme cases being augmented thirteen fold over the production from pure peat, and, without any admixture containing nitrogen, eleven fold.

In regard to its value for fuel, a subject now attracting so much attention, we have here data afforded by the various processes engaged in its preparation in this country and in Europe. The great question of its profitableness now remains to be solved by the many experimenters in this branch of industry, many of whom need the data and facts here brought together for a more intelligent direction of their labors. This is especially the case, as regards the comparative heating effects of peat and coal, upon which popular opinion is so erroneous. W. H. B.

8. *Recherches sur l'origine des Roches*, par Delesse, Ingénieur en chef des Mines, &c. 74 pp. 8vo. Paris, 1865. (F. Savy.)—Delesse has written much upon the origin of rocks, and whatever comes from his pen is deserving of study. Many important points are discussed in this memoir.

9. *Geology and Minerals: a report of Explorations in the Mineral Regions of Minnesota during the years* 1848, 1859 *and* 1864, by Col. Charles Whittlesey. 54 pp. 8vo. Cleveland, 1866. Printed by order of the General Assembly.—We barely announce this memoir, as one containing many facts of value on the subjects of the surface features of the region mentioned, the phenomena of drift, and the distribution and features of some of the rocks.

10. *Carte Géologique du Department de la Seine*, publiée d'apres les orders de M. LeBaron G. E. Haussmann, Senateur Préfet de la Seine, conformément a la déliberation de la Commission departmentale et exécutée sur la carte topographique, gravée sons la direction de M. l'ingenieurs en chef des Ponte et Chaussees, par M. Delesse, Ingenieur des Mines du department de la Seine. 1865.—We defer our notice of this very beautiful and complete geological chart to another number.

11. *Chambers's Encyclopedia.*—Parts 107, 108, 109, 110 of this excellent Encyclopedia have just been issued by Messrs. J. P. Lippincott & Co., Philadelphia, carrying the work to the word "Syntax."

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